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THE AMERICAN JOURNAL OF PSYCHOLOGY

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No. 1

INTELLIGENCE AND IMITATION IN BIRDS; A CRITERION OF IMITATION

By JAMES P. PORTER
Clark University, Worcester, Mass.

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I. INTRODUCTION

If in reply to the question "Do birds imitate one another?" the writer should answer merely in the affirmative, what would such a statement mean? To most people it would mean that birds individually seeing other birds perform a certain act, say pulling a string to open a door, copy this act more or less consciously and purposefully with a definite end in view. Such a reply would be understood to mean that all imitation is identical with that which conscious or possibly self-conscious adult human beings find in themselves. Perhaps a more schol-

arly and certainly a more academic response to such a query as the above would be to ask in turn "what kind of imitation do you want to know about?" With such a way of acting, or of getting things done, as that of imitative functioning which is felt to occupy such a large place in both animal and human mental development there are very probably different stages or levels. Indeed, these stages may be so well marked as almost to demand the assertion that there is imitation of different kinds. Without, however, maintaining that the results which we have obtained in our study of birds require us to go quite so far, it is hoped that the present and later papers will afford ample proof of the fact that in learning an action through imitation many species of birds do not imitate in the same way as human adult.

No student at all in touch with recent tendencies and problems in Psychology or in the study of Animal Behavior can question for a moment the possible practical value of a study of imitation in animals. The work of the student of animals will certainly be found to be valuable for the Experimental Pedagogy of which we hear so much at present. Not so much that direct application of conclusions will be possible as that methods and conditions of experimentation which one must perforce use with animals in order to determine accurately just the factors involved will be found at least of suggestive value to the student of Child Psychology, Experimental Pedagogy, and, most of all, Social Psychology. For some years the writer has found the principle of "trial and success" or "trial and error," as it is most commonly called, one of the most fruitful with classes in Social Psychology. It is the critical use of this and the results obtained by students of Animal Behavior, particularly on Instincts and Imitation, which constitutes one of the excellent points of that recent book by Davis on Psychological Interpretations of Society.¹

The following may serve to indicate further the writer's feelings as to the probable bearings of the present studies of imitation in animals: Professor Royce, in "The Imitative Functions and their Place in Human Nature," *Century Magazine*, 1894, says, "As regards the uses and the results of the imitative functions in human life, the foregoing general indication of their breadth and depth is only the merest beginning of a comprehension of the part they play in our education and

¹ Professor McDougall's book, "An Introduction to Social Psychology," is in the first part very promising from the genetic point of view, but the later discussion is, to say the least, somewhat disappointing. Professor Ross's *Social Psychology* makes little attempt to analyze definitely either Suggestion or Imitation, much help in which is to be had from the study of animals as well as children.

in our consciousness. It is not because they are common, or because they are, in deepest origin, partly instinctive, that I lay such stress upon them. It is because they are in their proper and almost inextricable entanglement with our individual or temperamental functions, absolutely essential elements of all our rationality, of all our mental developments, of all our worth as thinkers, as workers or as producers; it is, too, because of this value of imitation as the necessary concomitant, and condition, and instrument of all sound originality which is still so inadequately understood by teachers, by critics of art, by students of human nature generally—it is on these accounts that I deem the study of the imitative functions probably the most important task in the psychology of the immediate future.” In view of the analogies which we may safely infer to obtain between at least the higher animals and man, it is not too much to believe that the study of Animal Behavior may rightfully assume its part of this task.

Several years ago the writer made a series of tests on two house mice which were captured in order to prevent their interference with his work with the birds. They, of course, liked the bird food, and, indeed, in one instance one mouse went so far as to open the door before the birds came down to the box.

Having taken prisoner two of these offenders I decided to test them with apparatus similar to that used by Small in his experiments I and II (*Amer. Jour. Psy.*, XI, p. 135). The results may be gathered into a table like the following:¹

TABLE I

Results of tests with two House Mice giving time taken for the larger or smaller, or both, to find a way through the sawdust into a box in which food was placed. The time in this as in all the later tables is given in minutes and seconds.

Trial	Time	Order of Entering
1	17:	Smaller—Larger.
2	1:30	Larger—Smaller
3	2:	Larger
4	1:	Larger
5	:45	Smaller
6	2:	Smaller

The average time for the remaining thirteen tests does not fall below that for experiments 2-6 inclusive. There is the same alternation as to which first enters the box until the fifteenth test when the smaller enters first for the remainder of the series. There may be some interest for the reader in the

¹These experiments were made in the Psychological Laboratory of Indiana University.

fact that according to my observation it required something like thirty to forty-five seconds for the mice to become adapted to the light. This came upon them suddenly when I raised from over them a second box in which they habitually stayed. As a rule they began to move about only after the lapse of this length of time.

Three tests were made on the first day and four each day following. After the seventh trial the mice could not have been very hungry, as bread crusts were inside their hiding place or on the floor of the cage. Meat or most often cheese was placed inside the sawdust box as a food more to their liking and as an incentive for their digging through.

So far as this single series is concerned, the mice learned more rapidly than the white rats tested by Dr. Small with the same apparatus. The initial times are shorter and the reduction of this to a minimum is more rapid with the mice. The fact that the mice had no hiding place except as they dug their way through the sawdust, may, in connection with their greater wildness, explain why they learned more rapidly than the white rats. The minimal time is about the same for both animals. The time required for adaptation of the eyes to the light is a rather constant factor for all the trials for the mice.

Another box with a zinc door held closed by narrow paper strips secured by wax, an apparatus very similar to another of Dr. Small's boxes, was used with the mice with practically the same, though less uniform, results.

We may ask (and this is the chief reason for introducing these results just here) how are we to determine to what extent and in what sense one mouse was imitated by the other? In several of the tests the one following seemed to dig in through the banked-up sawdust in its own way. Could the action of the other be then of any more than of the merest suggestive value? Was it merely the *following instinct* which is so prevalent in many species of animals and to some extent in man? Did one use the odor left by the other and thus have a stimulus at every step? If an odor stimulus does guide the animal at each turn, then it is not following a copy furnished by another, though the human observer would be unable to distinguish what was imitation and what was not.

These mice, as well as Berry's White Rats and Cats, and also the Raccoons studied by Cole and Davis, may all use the odor left by the animal which is acting as the model. This would seem, therefore, to suggest that all odors should be thoroughly removed from the apparatus before the animal to do the imitating is allowed to try. Spaulding, in testing the learning process in Crayfish, had to do this and all would agree

that to do so for tests of imitation would be as difficult as well as more necessary.

Birds would therefore appear to be very good subjects for a study of imitation since at least the higher ones have the olfactory lobes really very little developed and they probably use the sense of smell to small extent. For them the sense of sight is predominant as it is in man. In fact a good argument might be made to show that it plays even a larger rôle with birds than with almost any other animal. Edinger's work has shown how the keenness of vision, the large eye and the optic lobes enable them to see acutely and from afar. It is clear, then, that a careful estimate of the sort of contribution made by the separate sense fields is a prerequisite of the study of imitation. But what advantage is there in an animal which uses more exclusively the eye than the sense of smell as the source of stimuli which guide it to action? Obviously if our experimental apparatus were devised to appeal to the sense of smell, and if we as observers could detect that the animal, instead of acting on the odor stimulus, responded rather to the model set by another, then the odor guided animal would be the equal of those in which sight is predominant.

It is probable, however, that the eye requires in a less rigid way the following instinct. Certainly the criterion of the presence of imitation almost universally used by students of imitation, both human and animal, has shown that what the imitating animal sees done is thought to be all important. It is commonly stated that such an animal seemed to see what the animal to be imitated was doing. Seeing this, it did not work with the apparatus as it had previously done, but changed its method and followed the copy. Certainly such a standard as the above requires that the animal use the sense of sight and the more certain we are that keenness of vision is possessed by the animal the better prepared are we to start with.

Very few species of birds exist that are not the natural prey either of their own kind or other animals. To escape their enemies or to capture their prey very sudden and rapid action is absolutely necessary. The sensing of stimuli from afar and quick response is one of the most striking characteristics of birds. Can one bird imitate another and thus obtain food and escape enemies? If so, then we have in a way what may be called "vicarious functioning" to which, in fact, all imitation is equivalent. True we may find in birds no higher kind of imitation than the *following instinct*. Bird migrations and their other sorts of gregariousness in addition to the demand for sudden response mentioned above—all these emphasize the value to them of instinctive imitation. Illustrations of this

kind of imitation come very readily to mind. One bird starts to fly and the rest follow, one chick drinks and others do so at once.

The problem, then, of the present study is as follows: Can we by experiment and observation demonstrate that in these favorable subjects, the birds, facts are to be found which unmistakably point to imitation of a higher kind than the merely instinctive or the *following instinct*?

II. METHODS OF EXPERIMENTATION

The purpose of the experiments with which we are here concerned may be said to be twofold. First, to obtain a measure of the rate of learning in as many different species of birds as possible. Secondly, to determine if and just how birds of the same and different species imitate each other.

One would naturally expect that conditions favoring imitation might very well interfere with a uniform rate of learning, and *vice versa*. If two birds fight or pay attention to each other, then neither can at the same time be working to open a box by pulling strings. Nevertheless the writer regards the second aim as more important than the first. At least the present study has been made from this point of view. Results on rate of learning in birds have already been obtained in sufficient number to make later ones chiefly corroborative in value.

The method throughout has been to place on the floor of the large cage in which the birds were confined a small box to be opened. The results in series A, B, and D were obtained in an in-door cage twelve feet long six feet wide and six feet high. The top and one side were covered with white muslin. The other side was formed by the wall of the room in which there were two large windows. The two ends were of wire mesh. The floor, the window sills (the floor being on a level with these), and posts were covered with white paper in order that as much light as possible might be had for taking photographs. All photographs taken in in-door cages have been taken with the camera concealed in an inverted box placed on the floor of the large cage. This box being left in the cage all the time, the birds were quite accustomed to it. It was found that for most days in midwinter with a rapid lens and plate very satisfactory results could be obtained in 0.02-0.01 sec.

Extreme hunger has been carefully avoided. The small box most used was the same as that used with the Cowbirds and Pigeons the description of which may be found on p. 257, volume XVIII of the *Amer. Jour. Psy.* This box is twenty inches long thirteen inches wide and twelve high. From Fig. 1 it may be seen that the door is placed in the middle of the front on which last and the left end is a wire mesh covering.

The door is opened by pushing or pulling in any direction any one of the four strings which in Fig. 2 are shown in the niche to the right of the door. It is easily evident that the door may be opened in several different ways. It is thought that this seeming objection in actual experimentation proved a decided advantage with some birds.

All the birds are first fed by means of food placed in this box for some time before they are required to open the door. In making an experiment the food is placed on the inside just beyond the wires. The door is then closed. The birds are free at all times to move around the box. No bird has ever been confined in order that it might see another first open the door and then be liberated and allowed to try for itself. Such a method requires a break in the time which is certainly more difficult for the animal to cross mentally than it is for man.

As will be evident in many of the later series, the experimenter has sought whenever possible, by change to a kind of food more to the liking of some birds present in the experiment, by change in the positions of the food-pan, by changes in location of the strings on the box, by the introduction of strange, more pugnacious, younger, or more active and playful birds; by all these and other means the experimenter has sought to produce rivalry, competition, struggle, fear, new caution, interest and attention and above all a real necessity and opportunity for each animal to do something. When these conditions are obtained (and even then) our laboratory conditions are far enough from matching those in the birds' free life outside.

Some of the series may seem to the reader to have been carried to unnecessary length, yet such tests as laboratory studies permit are very foreign to birds and should, therefore, be given many repetitions. How foreign and artificial one can scarcely appreciate unless he has sat by the half hour outside the cage looking through the peep hole and watched the birds in the early tests doing everything but pulling or pushing the (to the experimenter) perfectly obvious string.

It is probably more natural for one bird to imitate another in song, or for a parrot to imitate the human voice, and yet how long often does one have to wait for results. Many repetitions of a word are often required before we expect to get any attempt at imitation. Artificial tests, then, can hardly err on the side of supplying too many opportunities for one bird to see another perform the act which is the model.

It should be added, as well worth the time and patience required, that such changes as those mentioned above, particularly those of the location of the strings, are better calculated than almost any other to put to a real test the intelligence and

power of analysis possessed by these and other animals. In the experience of the individual animal, as in human experience, there is a constant recurrence of old situations with a slightly new aspect. Tests of the power of inhibition, of analysis, as well as imitation, the writer is constrained to believe find favorable conditions as much in varied relations of the same apparatus as in refinement and multiplication of new apparatus. The same food box used with most of my birds may seem to the reader an undue adherence to the rules of scientific experimentation, but the results we trust will abundantly justify the method adopted.

The sub-title of the present paper, "A Criterion of Imitation," has reference directly to a point of method rather than that of apparatus. In an earlier paper the writer obtained some results of interest from the standpoint of Imitation when a change was made in the fastening to a box and both English Sparrows (the one accustomed to opening the door in a now impossible way and the other never having opened the box) had opportunity at the same time to do something. The first tried the old method several times in the presence of the second whereupon the second gave some signs of imitation of a rather blind and impulsive kind.

Early in the present work the writer began to make use of a certain standard or criterion of the presence of Imitation which may be stated somewhat as follows: Bird No. 1 is induced to open a box which may be done in one of several different ways. Bird No. 2 by the means indicated above is allowed to supplant No. 1. The effect of this different method of opening on the behavior of No. 1 is closely observed and recorded. The behavior of No. 2 will rarely be identical repetition. We may be fairly certain, then, that No. 1 will have furnished to him by No. 2, or *vice versa*, an example or act to imitate.

Such a criterion has certain obvious advantages. Students of imitation in animals allow No. 1 only to try to open the box but never to succeed. They then allow it to see from some place of imprisonment, or other rather removed position, No. 2 do the act of opening. No. 1 is then released to see if he will do as No. 2 did. They by this method almost certainly get less of rivalry, effective interest and attention, or of that condition of the mental which in man is so often the precondition for the performance of the imitative act. This condition is probably most often brought about through the fact that the animal or human has had previous experience with, or similar to, the act to be imitated.

Let us consider briefly a concrete case or two, more of which we shall see later. The Old Crow left at once the end of the food box, to work as the young Crow had just done on the front

side, the strings which the Old Crow had pulled scores of times in earlier tests. Would he have done so without his previous experience? This Young Crow showed signs of imitating the Old Crow most often only when the efforts of the latter were similar to those previous efforts of the former which had resulted in opening the door and thus obtaining food. English Sparrows, Cowbirds, Bluejays, Baltimore Orioles, and Crows when submitted to experiment by negative as well as positive results, exhibit behavior which makes it advisable to use the criterion as stated above. If bird No. 1 has not previously opened the box it is unlikely that it will try vigorously to do so unless the experimenter can in some way delay action on the part of bird No. 2. But, if No. 1 has previously obtained food in its own way it not only will be more likely to struggle, fight, etc., with bird No. 2, but has given the experimenter, by its previous behavior, a standard by which to judge its present behavior. As a working hypothesis and standard of interpretation the reader is asked to hold in mind throughout this paper the criterion as stated above.

III. RESULTS OF EXPERIMENTS

Series A. English Sparrows and Cowbirds

These birds, several pairs of each species, were forwarded to me in the same small box. While the large cage in which they were to be kept when experimented upon was being built they were placed in smaller cages and supplied with plenty of food, sand and water. It was interesting to see that the Cowbirds began at once on being liberated to bathe in the large water pans while the Sparrows showed a strong preference for the sand bath.

The Cowbird is much easier to keep in good condition than the English Sparrow and, I believe, than most other birds. Early in my work it was noted that the Cowbird was very attentive to the Sparrows. See Fig. 1, A. It soon developed that they were looking for parasites. Judging from the fact that I have never seen a single parasite really taken by them one would conclude that we may have to deal here with a very strong habit or probably an instinct. They look over very carefully the perches, window sashes, etc., in fact all objects where parasites might be found. It is probable that many times the Sparrows need the good offices of the Cowbird; but very early the male Sparrow learned always to keep a safe distance away. The little female Sparrow was able whenever she wished to so use her beak that the Cowbird refrained for a time from coming too near. Fig. 1 B. It is not impossible that a part of this behavior of the male Cowbird was due to the mating instinct.

After a few weeks during November the male Cowbirds began to strut and "gobble." I use here the terms for the rooster and "gobbler" because his actions and notes were strikingly similar to these, particularly the latter. Such actions were continued for two or three weeks at a time when they would cease to reappear again some weeks later. In addition to this "gobble" which is very like the call of the Red-wing Blackbird, only not so loud and clear, my male Cowbirds further expressed their good feeling by a series of notes, low, liquid and, indeed, very musical. It needs only to be heard to be appreciated and enjoyed. Especial emphasis is made of this matter of song for the reason that it has not been heard by many. The female has only the low rough call characteristic of all the Blackbirds.

This series (Series A) was begun on November 11, '06. The box was the same as that used in earlier work and described in my "Further Study, etc.," p. 257. The niche between the posts to the right of the door is designated as Place 1, the niche to the left as Place 2, niches 3 and 4 are on the left end, and niche 5 on the rear right corner. Throughout this paper the cords placed across each of these niches are referred to as string A, B, C, and D, string A being the first from the bottom.

In the first trial the female English Sparrow was successful, but only after thirty-one minutes and one hundred and thirty-nine efforts. These last statements are only in a small way indicative of the energy she expended. The "efforts" were simply those attempts which were sufficiently definite for me to count, and consisted chiefly in hops upon the left end and

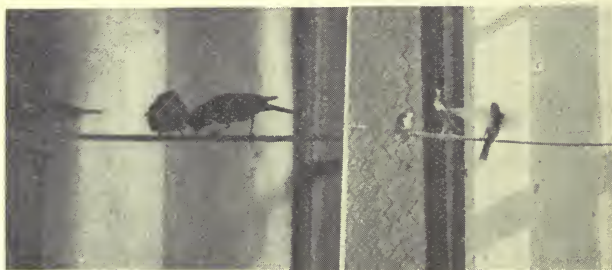


FIG. 1

A

B

The Cowbird looking for parasites on the perch and the female English Sparrow.

The female English Sparrow has resented the Cowbird's looking her over for parasites. The male English Sparrow is just alighting on the perch.

front of the box. As may be seen in the accompanying figure (Fig. 2), she opened the door in one of her first trials by

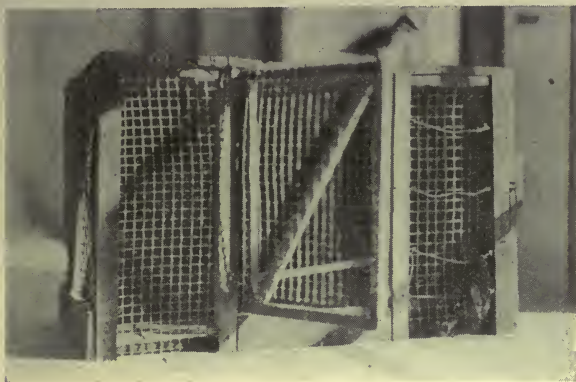


FIG. 2

The female English Sparrow (at the lower right hand corner of the box) opening the door by one of her first accidental methods. Many efforts of other kinds preceded this.

crawling in between the wire and strings and accidentally pulling on string B with the back of her head. In Fig. 3 is shown a way of opening the door which is, if such is possible,

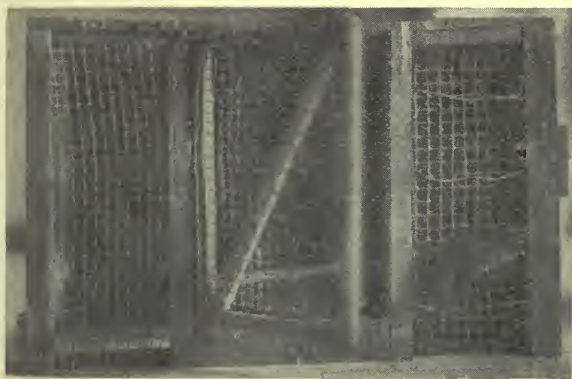


FIG. 3

The same English Sparrow as in Fig. 1 opening the door on the next day. She did not know what she had done until after she returned to the floor.

even more accidental. Here she is pushing down on string B with her head turned away from the door. She did not know

what she had done until she reached the floor and turned round. A little later in the series she hopped from the top and front edge of the box, turned as she did so, and seized the top string, D, in her claws. This is shown in Fig 4. This,

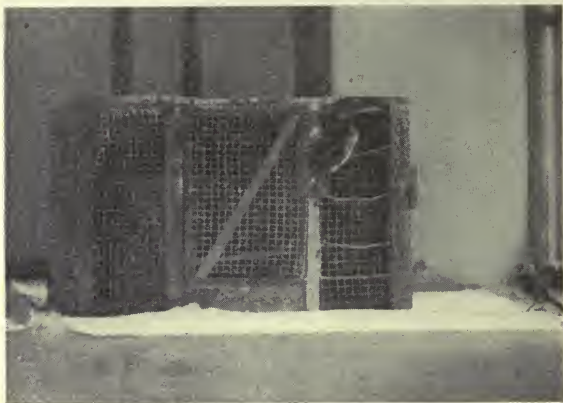


FIG. 4

The same bird as in Fig. 1. String D more directly worked with now than formerly.

with slight modifications, to be pointed out later, came to be her fixed method of opening the door. And yet she tried this

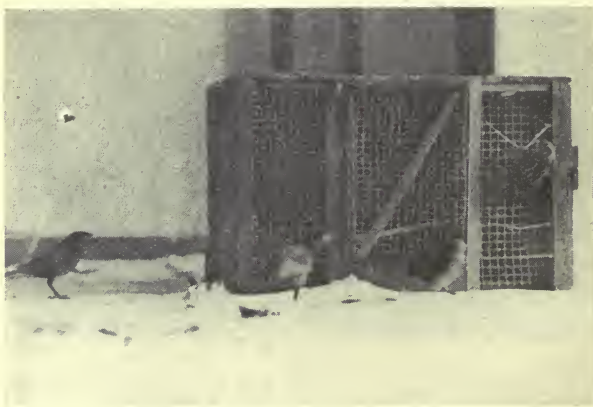


FIG. 5

The same female English Sparrow using the beak in addition to her claws. The door was rather refractory. The other birds were certainly more interested.

method only after many failures to reach the desired result through efforts very similar to those represented in Figs. 1 and 2. The reader will note that in Fig. 5 she is shown to be using the beak on the string in addition to the pull by the claws. This was done rather often when the door was a bit refractory. In other photographs not here reproduced it is shown that in the act of opening the door she scarcely stopped flying.

In one of his books C. Lloyd Morgan has called this method of animal learning that of "trial and error." This phrase has long been adopted by students of Animal Behavior. The reader will at once see that in the above series of photographs the writer was fortunate enough to obtain from this initial series a succession of this "hit and miss" or "try, try again" method purely accidental at first with the simplest and most direct act gradually selected. There is no question but that it is distinctively the animal method of learning. It means that successes are followed up because of the reward or pleasure (food in this case) which they bring. It means failures or less direct methods are discontinued because of the lack of satisfaction or pleasure.¹

In the accompanying table are recorded some of the results of this first series of experiments. For lack of space many of those results which merely show a reduction in time required are omitted. With this and succeeding tables the reader is to infer that no significant change occurred in those cases which are omitted. The times are given in minutes and fractions thereof, and the number of errors is also given. Attention may at once be called to the fact that the times are not so rapidly and uniformly reduced to a minimum as in earlier experiments with the same birds, and as will be shown in later experiments with different birds, but it must be kept in mind that the number of efforts and not the time is the better measure. And also if this better measure does not show rapid learning, it should be remembered that rate of learning is not that in which we are primarily interested. Imitation is our first interest in these tests. It will be seen many times before we conclude, that a longer time or many efforts, extreme cau-

¹ The writer feels impelled to suggest that if Principal Morgan's phrase were revised to read "trial and success" instead of "trial and error" it would much more satisfactorily express what is here needed. The words "trial and error" express the unpleasant, the non-satisfying, the negative aspect of these responses while "success" could be expected to carry with it more directly the pleasurable and positive aspect. Some may point to the unwisdom of such a change when the phrase has become one of our stock-in-trade. The answer might be, however, that our science of Animal Behavior is still in its earliest infancy when changes are most easily made.

tion, lack of pugnacity, lack of fear, and many other apparent disturbances may be just the condition which shall allow the other birds, those not opening the door, to give evidence that they have the capacity to learn vicariously or through Imitation.

In the present series the female Sparrow was not in normal condition much of the time. The male Sparrow was very wild. The Cowbirds were not nearly so active as the Sparrows and usually made very few efforts. The later photographs show, however, that they learn to be present when the door is opened. They are certainly more attentive and interested if behavior is any criterion.

TABLE II

Results obtained with a pair of English Sparrows and Cowbirds.

Trial	Date	Time	Bird opening Box	Efforts by Bird opening box and other behavior
1	11-16	31	Female Eng. Spar.	140
2	"-17	12	"	56
3	"-"	23	"	Not hungry.
4	"-18	2	"	1
5	"-"	8:45	"	
6	"-19	5:40	"	3
7	"-"	1	"	4
8	"-20	3	"	6
9	"-21	:35	"	1
10	"-"	8:25	"	23
11	"-22	5:20	"	1
12	"-23	1:05	"	1
13	"-"	:50	"	2
14	"-24	:50	"	2
15	"-"	1:15	"	3 On strings after door was open.
26	"-30	11:10	"	2 Escaped from cage and had to be caught.
27	12- 1	13:45	Male Sparrow	14 Not certainly imitation.
28	"-"	5:30	"	May be done in similar way due to great fear.
29	"-32	2:45	Female Sparrow	1
32	"-"	36:15	Male "	20 Female not hungry because she escaped from cage and found food.
33	"- 4	10:20	Female "	2
60	"-21	:17	"	1 On string once after door was open.
61	"-22	3:00	"	1 In poor condition.
65	"-31	12:35	"	1
66	1- 1	2:25	"	1 Male apparently imitating a good deal.
71	"- 5	4:10	"	9 Strings to opposite side of door, place 2.
72	"-"	6:00	"	11
73	"- 6	6:00	"	6
74	"-"	1:50	"	1
75	"- 7	:35	"	1

TABLE II—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by bird opening box and other behavior
81	1-10	3:35	Female Sparrow	3 In poor condition.
82	" "	:25	"	1
83	" "	:40	"	2 Strings to left end, place 3. See Fig. 8.
84	"-11	1:33	"	1
85	" "	:12	"	1
94	"-14	:23	"	1 Strings to rear on left end, place 4.
95	" "	2:10	"	5
96	"-15	:16	"	1
103	"-19	1:08	"	1 Rear side of box changed so that there are posts and wire there. Disturbed.
104	" "	:12	"	1
105	"-21	:18	"	1
111	" "	:05	"	1
112	"-22	:36	"	1 Strings now on rear side, right corner, place 5.
113	" "	:15	"	1
114	" "	:17	"	1
125	2- 1	:50	"	1
126	"- 2	:26	"	1

NOTE. The results omitted from the above and following tables do not add anything to the typical results which are here given.

Very early the female Sparrow was seen to take her position on the front edge of the box and hop on the strings although the door was already wide open. Most often, of course, she did not do this. The occasion for this needless repetition lay in the fact that both Cowbirds early in the series came to be, with respect to food, what they are in the wild in the rearing of their young,—a kind of parasite. They waited until the door was opened and then went in to eat. They were nearly always the first to enter. There were often pitched battles in which the little female Sparrow was often victor. But the male Cowbird was on the whole too large for her. If he entered first, and soon left she approached the box and, this first link in the chain of reactions having been performed, the others followed; she pulled the string with the door already open. She did this on the 11th, 12th, 15th, and 60th trials. In earlier work I found the Pigeons and Cowbird doing the same thing. As will be seen later, it has been observed with nearly all of my birds. Taken in themselves such errors are perhaps of little value. As indications of the degree to which the bird analyzes the apparatus it is working with, as an index therefore of the extent to which the animal is conscious, they are most significant and important.

It should be noticed that the male Sparrow on the 28th and 32nd tests opened the door. He actually did it by hopping up

on the strings from the floor. As he was very wild, he never really alighted on the strings. He made many trials that were very similar to those of the female Sparrow. For the reason that she was not in good condition, her attempts to open the box were much delayed. Thus an abundant opportunity to open the door was offered the male.

Both his unsuccessful and his successful attempts, certainly the former, would, according to previous standards of imitation, without doubt be classed as due to imitation. It seems best, however, to delay such a classification until our tests have multiplied in number and other birds of the same and different species have been tried.

It has always seemed to the writer that students of Animal Behavior, particularly those who work with the higher animals, were in danger of overdoing the simple tests which deal only with simple association processes. As Professor Mills has pointed out, our tests should show the power of the animal in question to inhibit old associations and form new ones. Many of the series described will therefore have to do with changes in the location of the strings such that the above powers will be put to the test. Furthermore such modifications will also serve the very useful purpose of giving those birds expected to do the imitating the opportunity to manifest any tendency which they may have in reality to do so.

Accordingly after a series of seventy-eight tests during the second half of which the errors of the female English Sparrow were very few, the strings were changed to Place 2 on the opposite side of the door. What, now, is the effect on her behavior? In the part of the table following, "strings to opposite side of door," we see that at first she made numerous errors. But it is of interest to note that four of the nine errors in the first trial are on the new side, and are made because more force is required to open the door now. Of the eleven errors in the second trial, six are on the new side. In the third trial she went but twice to the old side, where there were no strings. In Fig. 6 we see that she came to use the beak in this series as well as in the first. It is only fair to state that about one-half the above errors are clearly due to the fact that her weight was not enough to pull up the latch. In fact by the third trial she had learned to wriggle while seizing the string in her claws. The left side of the door or this part of the box now seemed to be the more interesting to the male English Sparrow.

In that part of the table immediately following "strings on left end at front corner" (Fig. 7) we see that all eleven trials are without error except the first where there is but one. My notes record the fact that she inhibited the impulse to jump on the old positions many times. So far as "errors" are con-



FIG. 6

The female English Sparrow of Figs. 1-5. Strings now changed to Place 2. She used the beak here also. See Table II or the text for the readiness with which this change was made.

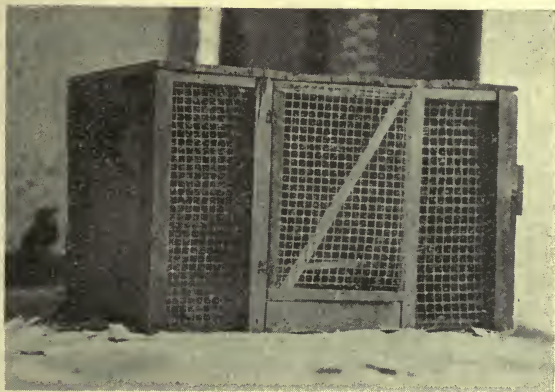


FIG. 7

Strings are shown in Place 3. See Table II or text for the way in which she learned this.

cerned, this series then, is almost perfect. Practically the same may be said of her behavior with respect to the remaining two changes,—“strings to left end at rear side” and “strings to rear side at right corner.” There are no errors at all in the last series and the errors in the second trial of the last series but one are almost certainly due to the fact that she had something wrong with one foot.

Some interesting conclusions follow from what has just been given. It is evident that after the first change in the location of the strings, these come to stand out from the other parts of the box; they are distinguished. They are singled-out-elements of a compound, and we seem to get some little beginnings of what in human minds is called "abstraction." But let me hasten to say that the reader must not think that such abstraction is conscious in at all the degree in which we find it in adult human behavior. As has been suggested above, and as will be shown later, the *cause and effect* relation probably never is felt or thought by these birds. All that is meant is that some one part, rather than a definite place to hop upon, gradually comes to be felt apart and thus impulsively to be worked with more than some other.

Series B. Male English Sparrow and Female Cowbird

These birds are those which have been with the female English Sparrow during all the previous tests. The male Cowbird had been removed much earlier for the reason that it cost the female Sparrow too much of her energy to fight with him.

It will be seen from the table below (Table III) that the rate of learning is very slow with these two birds. The times are often very long and the errors for the English Sparrow many. This is chiefly due to his extreme wildness, and were it not probable that this very wildness really plays into the hands of the experimenter the present series should be counted in large part as a failure.

It is possible that being so long accustomed to seeing the female English Sparrow open the door they now sit and wait. They have learned *not* to do instead of to do. Thus they fail in thirty minutes in the first trial and the door is opened in the second trial by accident. In the third the male Sparrow succeeds in thirty-one minutes after many trials. The manner in which the door was opened by him is of interest. The uncritical observer might call it imitation, for he flies out from the front upper edge, turns, and in his flight seizes the strings. This is very similar to the method of the female Sparrow, but I have had frightened Sparrows behave in this same way when they were alone with this box. Furthermore I have observed this reaction in the English Sparrow as it seized a feather or bit of other material in its free life out of doors.

In the fourth trial both fail in one hour. But this apparently negative result really means quite different behavior by each of the two birds. The amount of activity and therefore the energy expended by the Sparrow is enormously greater than that of the Cowbird. The former shows signs of knowing where to alight, though he cannot overcome his fright. The

TABLE III

Results obtained by continuing the tests with the male English Sparrow and female Cowbird which were in the preceding series.

Trial	Date	Time	Bird opening Door	Efforts by Bird opening Door. Other behavior.
1	2- 3	30:	Failed	
2	"- 4	58:	Male Sparrow	Many efforts. Out from top, turned and near strings.
3	"- 6	31:	" "	58 of many kinds.
4	"- 7	60:	Failed	Sparrow much more active.
5	"- "	20:45	Female Cowbird	
6	"- "	19:45	" "	Sparrow many attempts. He worked all the time.
7	"- "	55:55	Sparrow	Very wild.
8	"- 8	49:30	"	Many movements like those of female Sparrow. Has known what to do almost from the first.
14	"-12	4:30	"	So wild that did not eat inside box.
15	"-13	20:20	Cowbird	Sparrow made 36 efforts.
16	"- "	44:45	"	2 Sparrow made 77 efforts. He was not well.
19	"-15	45	Failed	
20	"- "	1:30	Cowbird	She made very few efforts.
21	"-16	2:45	"	When ready she walked up and pecked string. See Fig. 8.
22	"- "	1:30	"	
30	"-21	26:	"	
31	"- "	4	Sparrow	
32	"-22	26:30	"	With beak as Cowbird did.
33	"- "	9	Cowbird	
40	"-26	1:40	"	
41	"-27	11:20	Sparrow	
42	"- "	9:20	Cowbird	Sparrow struck top of door as Cowbird had done.
45	3- 1	25:45	Sparrow	He pulled string with bill.
46	"- "	6:40	"	" " " "
47	"- 2	12:40	Cowbird	
48	"- "	18:45	Sparrow	" " " " Fig. 9.
49	"- 3	6:30	"	He tried his own method many times.
50	"- "	6:10	"	He continued both.
51	"- "	1:05	"	" " " "
52	"- 4	23:45	Cowbird	
53	"- "	4	Sparrow	Combination. Fig. 10.
54	"- "	7:45	"	From the floor as Cowbird.
55	"- 5	17:15	"	" " " " "

TABLE III—*Continued*

Trial	Date	Time	Bird opening Door	Efforts by Bird opening Door. Other behavior.
56	3- 6	13:30	Sparrow	Combination.
57	"- 7	11:50	"	"
58	"-11	5:20	"	Cowbird often not down at all.
73	"-"	9:55	"	Female Sparrow present.
74	"-19	1:45	"	She disturbed considerably. He followed.
77	"-22	14:07	Failed	
78	"-23	13:20	Sparrow	
79	"-24	12:40	"	Another female Sparrow present.
95	4- 2	12:40	Cowbird	Strings to left of door.
96	"-"	10:40	Sparrow	As Cowbird did and as he had been doing for some time.
97	"-"	24:10	"	
98	"-"	4:20	Cowbird	Sparrow so wild he would not go in box at all.
99	"-"	11:40	"	
100	"- 3	4:25	"	
104	"- 5	9:10	"	
105	"- 6	3:45	Sparrow	
106	"- 7	38:55	"	
107	"- 8	5:15	Cowbird	
108	"- 9	7:45	"	
109	"-10	18	Failed	

Cowbird was successful during the fifth and sixth trials, Fig. 7, but as a rule (and this obtained throughout this entire series) she waited until the Sparrow had made many attempts; then she came down and by a few pecks on one of the lower strings opened the door. For example in the sixteenth, eighteenth and nineteenth the Cowbird was successful, but the Sparrow made thirty-six, thirty-six and seventy-seven efforts respectively. Although he was the one to open the door in the trials just preceding these last, his great fright and wariness would not allow him to enter the box and eat the food. His habit was to seize the bits of food and carry them out to the floor of the larger cage and eat them there. It is interesting that my notes record the fact that the Sparrow seemed ill and had seemed so for a few days previously. The Cowbird then continued to open the door until the thirty-fourth and thirty-fifth tests. In the latter the Sparrow used his beak on the string, which was the Cowbird's way of doing, Figs. 8 and 9. Some ten tests later the Sparrow came to strike with the bill the projecting upper edge of the door, a sort of effort which the Cowbird had been using for some time.

In the 48th, 49th, 51st and 52nd tests the Sparrow again used the bill to strike the first or second strings and he stood

on the floor. See Fig. 9. These would seem to be modifications of his two earlier methods. One might reasonably infer that such variants were due to the example he had had in the behavior of the Cowbird. It should be added, however, that my notes describe the efforts of the two birds in a different manner. Those of the Cowbird are designated as "mumbles

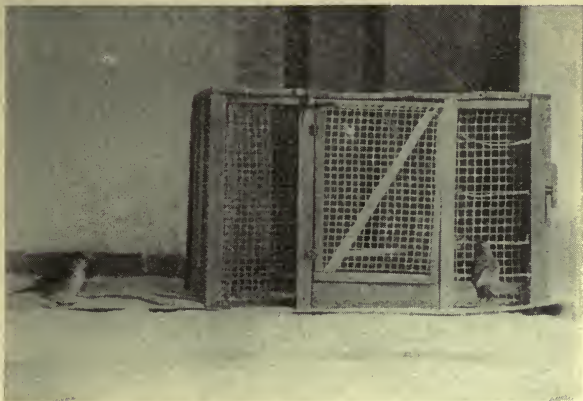


FIG. 8

The female Cowbird as she opened the door with the beak. This is a very simple way of opening the door. As a rule the male English Sparrow made many attempts before the Cowbird left the perch.

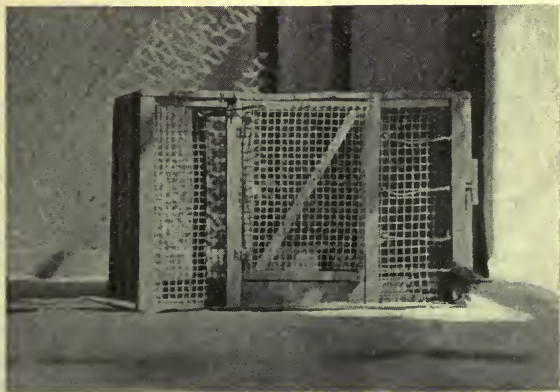


FIG. 9

The male English Sparrow as he opened the door following the times when the Cowbird had used her method shown in Fig. 8. He had been flying out from the top and alighting on the strings.

of the knots on the strings," those of the Sparrow as "pulls of the string."

In the 53rd test there occurred a change in the method of the Sparrow that should be of much interest to all students of animal and child life. Instead of using any of the methods he had up to this time employed he now alighted near the free side of the door and two-thirds the way to the top and struck the top string with his beak. A better way to express this

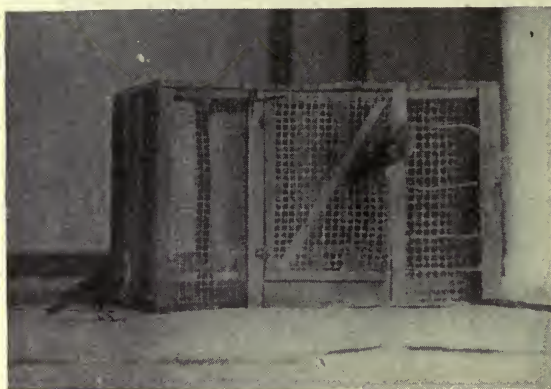


FIG. 10

The male Sparrow using his combination method. His fear seemed to be one of the chief incentives for this change from that shown in Fig. 9.

would be to say that after many attempts he succeeded in opening the door in this way. Fig. 11. In reality this method is to be designated as a combination of, at least, two of his previous methods. The writer would not have any one believe that this was a conscious invention on the part of this bird. He rather hastens to point out that such is very probably not the case. But neither are the first inventions of children consciously made. In fact such inventions as Professor Baldwin has cited and explained seem to be for the most part very similar to what is here recorded for the English Sparrow.

It was true many times that the female Cowbird did not come down at all until the door was opened. The Sparrow thus continued to open the door. A female English Sparrow was turned into the cage on the 77th test and a little later still another one. They served to distract him since he tended to follow them in their running around the box. If anything his fear which was constantly increasing was enhanced by their presence.

After the 100th trial the strings were changed to the left of

the door. The Sparrow pecked at the eyelets through which the strings passed on the old side, hopped up on the wire here and on the side where the strings were now. He seemed purposely to avoid hopping on the string. In spite of all his efforts he allowed the Cowbird to open the door in this 101st trial. But in the two following trials he was successful and performed as the Cowbird had done. In the remainder of the 115 trials he opened the door but once.

As a series to demonstrate animal stupidity the present would seem a good one. Yet as containing many results of interest to the student of imitation it is perhaps in many ways one of the best which the writer so far had obtained. There were many alternations between the two birds as to which one opened the door. The Sparrow's fright pushed him into many modifications and it so came about that he had a slow-going, timid but not wild bird as a model. Indeed, according to the writer's experience, it will seldom happen that conditions will arrange themselves in so favorable and fortuitously variable a manner as they did in this present series.

There are not many indications that the examples set by the Sparrows for the Cowbirds had any value whatever for any intelligent imitation. At the most they were only of general suggestive value and had their effect through the "following instinct." Such would fall under the head of instinctive imitation.

This is not saying that the Cowbirds are not capable of imitation of a kind higher than that which is instinctive. They may be, but not being at all as active as the English Sparrow it was not evident. Every one would agree that the manner of life of the English Sparrow for many hundreds of years has made them favorable subjects for such a study as the present one. Furthermore it should be asked,—Is it reasonable to suppose that a bird whose individual experience with this box is *nil*, or as little as that of the Cowbirds, could be expected to imitate beyond the instinctive level?

The effect on the Sparrows is quite different. They make readier use of the "following instinct" or instinctive imitation. But the chief question in this present connection is: are they able to use intelligent imitation, and in this instance was it imitation of the behavior of another species as well as that of their own? By intelligent imitation I do not mean the voluntary, reflective, intentional or rational. There may be something like the same difference between intelligent and rational imitation as has been long ago accepted between reasoned behavior and intelligent behavior. Intelligent imitation would thus require no reasoning, no very clear and definite idea of the end or re-

sult if any idea at all.¹ It would, however, demand considerable individual experience of the animal doing the imitating with the apparatus with which it is to work. The results so far obtained are too meagre to furnish us with a basis for generalization. They have suggested, however, the following questions as working hypotheses. Is it not rather futile to expect imitation above the instinctive grade of an animal without first giving it considerable individual experience with the mechanism it is to operate? Can we by allowing a bird to open a box in its own way establish a working basis for interest and attention so that if it sees the apparatus worked in a different and perhaps better way it will be led to add just a little more of the mental than is required in instinctive imitation?

A concrete illustration may help us here. The male English Sparrow toward the last of Series B, when the female English Sparrows were put in, ran round and round the box after them. This was instinctive. But he found it possible to open the box after the strings were changed to the left of the door only after the female Cowbird had done it once. If he followed her example even without appreciating the end (and I am inclined to think he did) then it is ever so slightly intelligent. At any rate we shall see what later series with many different kinds of birds will bring forth.

It would be natural to suppose that birds of any species would more readily imitate particularly in an instinctive way the actions of those of their own species. *A priori* a definite act which is to be imitated would be accompanied by more of general suggestiveness if done by one of their own kind. However, intelligent imitation based on so much of individual experience as was the case with this male English Sparrow, it would seem, need not be necessarily limited by the fact that the act is performed by a bird of another species. In fact it may be found that imitation of this sort is a mark of superiority of some birds over others depending as it probably does on a greater amount of activity and therefore having a better basis in a richer individual experience. It is not impossible that imitation of the instinctive kind is more natural and likely with the same species; intelligent imitation less so.

Series C. Results of Experiments with an Old Form of Food-box on English Sparrows and Cowbirds

This series of tests was made with the birds in a somewhat

¹ The writer does not maintain that the bird does not have reaching the food as an end but an imitative act *per se* must have in it as an essential part what we might term a proximate end. To be sure there may be several of these. To be in possession of these mentally certainly presupposes certain powers of analysis though these may be of all degrees of complexity.

smaller cage. This was only some six feet square, as tall as the larger, and built on the end of this larger one. Like the large cage, the floor of this was on a level with the window-sill. My chief difficulty was with the temperature. With the direct rays of the sun coming in through the one window and the fact that but one end of this cage was of wire, the heat became too great during the middle of the day even in midwinter. The results of this series contribute little positive with reference to imitation, but the negative results obtained are of suggestive value and interesting relations of the two species have been brought out.

As the reader may gather from the accompanying figure, Fig 11, the food-box here used is identical in principle with that which I first used. Fig. 1 *Am. Jour. Psy.*, Vol. XV, p. 319. Reference to Table IV will show that the female Cowbird first opened the door. In the next trial, though she made nine efforts, the female English Sparrow was successful. She did not pull the loop or strike at the free end of the latch, but pulled at the wire hinge which fastened this latch to the door. She drove all other birds away just prior to opening the door. It should be noted that after the door was opened in the first test the Sparrows did not enter, but ate the food thrown out through and under the wire by the Cowbirds. The Sparrow's method of opening the door cannot be called an imitation of that used by the Cowbird.

During the third test and some of the succeeding ones there were pitched battles between the Sparrows and the male Cow-



FIG. 11

Female English Sparrow opening door by pecking at the knot on the string where it passes through the latch. She repeatedly drove all the others away from the door in order to open it.

bird. Often the little female Sparrow dashed in and took food out of the Cowbird's beak in very much the same way as the English Sparrows, in their free life out of doors, may be seen to relieve the Robins of their choice worms.

At the eleventh test the string and wire were removed for the reason that the bird had now come to strike the latch where

TABLE IV

Trial	Date	Time	Bird opening door	Efforts by Bird opening door
1	2-22	23:40	Female Cowbird	7
2	"	6:40	Female Sparrow	3 Female Cowbird made 9 efforts.
3	"-23	26:50	" "	17 The Sparrow drove all others away.
4	"-24	10	" "	24 Struck some part of latch with the bill from the first.
5	"	9	" "	4
6	"-25	3:10	" "	4
7	"-26	3	" "	1
8	"	10:30	" "	18 Taking photograph a disturbance.
9	"-27	7:30	" "	8 Struck latch after door was open.
10	"	12:10	" "	9
11	"-28	2:35	" "	8
12	3- 1	23	Failed	4
13	" 2	14:55	Female Sparrow	1
14	"	20:50	" "	3 Not in good condition.
15	"-11	4:50	" "	9 Drove both Cowbirds away from any place where she wanted to get food.
16	"-12	10:20	" "	4
17	"	14:45	" "	2
18	"-13	1:50	" "	3
19	"-14	1:40	" "	1
20	"-15	3:25	Male Cowbird	By accident as he hopped off of box.
21	"	1	Female Sparrow	4 Two other female Sparrows present.
22	"-16	2:40	Female Sparrow 2	2 by Sparrow No. 2, 1 by Sparrow No. 1, and opened by No. 2, apparently imitation.
23	"-17	1:57	Female Sparrow 1	1
24	"	1:15	" "	4
25	"	3	" "	3 Boiled rice as food.
26	"-18	:40	" "	1
27	"	:15	" "	1
28	"	:20	" "	1
29	"-19	:34	" "	3
30	"	6:25	" "	4 Probably not hungry.
31	"-20	3:55	" "	4 Time and number of efforts fairly uniform after this to the close.
32	"	1:05	" "	1
66	4-10	6	" "	1

the knotted string passed through. For the twenty-first trial two other female English Sparrows were in the cage. The one designated as No. 2 was noted as showing most promise. In the following test the latter made a few efforts, then there was one effort by No. 1, after which very suddenly No. 2 opened the door by one well placed effort.

These results, which constitute all that were obtained during this series, are insufficient to serve as proof of imitation. Female English Sparrow No. 1 was too clearly the victor and in later tests drove away others of her kind as well as the Cowbird.

It will be seen that this series furnishes us with fairly satisfactory results so far as concerns times and efforts required. Toward the close the physical condition of the Sparrows did not warrant further trials.

Some weeks later an attempt was made to test a pair of Cowbirds and a Junco with the same form of simple maze which was earlier used with a Vesper Sparrow, a female Cowbird and several English Sparrows. See *Am. Jour. Psy.*, Vol. XVII, p. 253. It is not the intention here to state in detail the results obtained. In order to avoid the difficulty encountered earlier when some of the birds caught their claws in the wire mesh which formed the floor as well as the top and sides of this simple maze, the bottom was cut out. Typical results were obtained with each bird until they formed the habit of stopping at the far corner of the maze or that part which, because of the small size of the experimental cage, was thrown very near the window and on a level with it. The birds, instead of attempting to get through the maze, were attracted toward the source of light and thus stood still or struggled to escape.

This maze is therefore an unsatisfactory test for quantitative results when used under these conditions. In fact the writer had this feeling after having made the first tests.

*Series D. A Pair Each of English Sparrows and Cowbirds
and a Single Junco*

For some years the writer had wanted to make tests similar to those made with the English Sparrow on the Junco. The reason for this is that the Junco remains in this latitude throughout the winter and lives in small groups. When a friend kindly brought me one of these birds, I lost no time in beginning work with it. It seemed none the worse for having bumped against the side of our library building. I found that it was easily kept in captivity since like the Cowbird it seemed to do much better on the various kinds of seed and cracked corn than the English Sparrow. From facts like these one may infer that the English Sparrow's rapid increase in numbers is

not due to the fact that it is more hardy than other birds. When placed in the larger one of my cages with a pair of English Sparrows and Cowbirds, the Junco spent the time during the first tests running back and forth on one of the window sills.

The male Cowbird opened the door during the first trial of this series in a purely accidental way. In the second trial the male Sparrow was successful. He did it in much the same way as the male of this same species who has figured in the earlier series A and B. His fright seemed to induce him to use this method of flying up and out and seizing the string without alighting. In the third and fourth tests the female Sparrow was successful by hopping up on one of the strings. In the fifth the Junco succeeded by striking the string with one wing as it chased the female Sparrow away from the front of the box. A great change had come in the Junco's behavior after the first trial and previous to the fourth. It chased the female Sparrow away at every opportunity, but was in turn no match for the male Sparrow. It was evident that the female Sparrow wanted to open the door. The female Cowbird remained outside the box when the Junco was on the inside.

In the sixth test the male Sparrow made an effort. The Junco chased the female Sparrow away several times. The latter was induced to run round the box several times by following the male Sparrow. Finally the female Sparrow opened

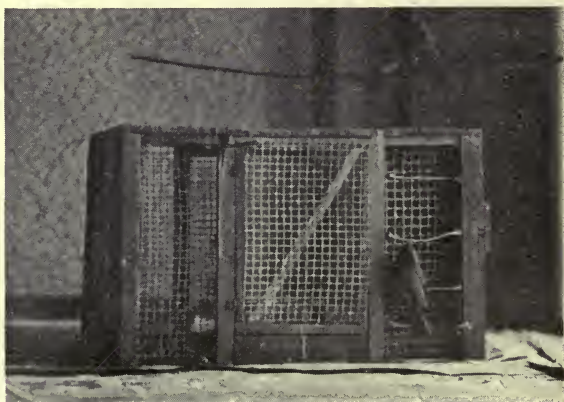


FIG. 12

The first method used by the Junco in opening the door. She must make many hops to this spot for the reason that her claws do not strike string B. The Cowbird is looking for parasites on the male English Sparrow.

the door and after the others were inside eating she jumped on the string once with the door open. In the seventh the Junco was successful, but the rival must first be chased away. Such control of the situation was not to last long, for in the fourteenth test the tables were turned and the female Sparrow proved now the better fighter. This as is stated in the table was probably due to the fact that she was not in good physical condition. It is often to be noted that such condition in the Sparrows causes them to be more pugnacious. It has been observed when these have had to chase the Cowbirds away.

The number of efforts required for the Junco to open the door was often very large and this continued throughout the whole of this series. The reason for this may be seen in Fig. 12. This bird often simply jumped astride String B without touching it. Frequently she must continue to jump until she does strike the string with one claw, usually the lower one. Certainly such a method does not point to a great degree of analysis of the task to be performed and which is performed. If she does this by imitation it is certainly of the blind sort. As to whether or not there is imitation the facts to be recorded now may give an answer.

In the seventeenth test the female Sparrow ran in from the window and made an attempt very quickly and just after the Junco had tried; when one approached the strings the other did so too. The rivalry was more intense.¹ In the succeeding test the Junco tried on the door at the same time and after the Sparrow was on the strings. Boiled rice had been placed in the box with the expectation that the female Sparrow would try harder. Such was really the case.

In the nineteenth test the female Sparrow would not allow the Junco to come near the box. During this test the reason occurred to me, for the fact that the female Sparrow finally opened the door by jumping on the top string, D. When she jumped lower than this there was space enough between the strings for her to alight without striking any of them, but not so at the top. The upper edge of the box crowded her down, as it were, so that in jumping to the wire she pushed in string D. This is strikingly similar in the lack of analysis of the relations of parts of the box to the Junco's great number of efforts, and prepares us to understand certain mistakes of the Bluebirds to be recorded later.

¹ The word "rivalry" is here used simply to indicate the struggle between these birds and does not imply that the writer regards it as the same as human rivalry.

TABLE V

Results of tests with a pair of English Sparrows, Cowbirds and a single Junco.

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior.
1	5-22	11:	Male Cowbird	Accidental.
2	"	22:	F. Eng. Sparrow	By flying up and while flying seizing string.
3	"-23	30:10	" "	Hopped on string.
4	"	13:47	" "	
5	"	3:	Junco	Struck string with wing in chasing Sparrow away.
6	"	5:53	F. Eng. Sparrow	3 Other Sparrow tried once.
7	"	4:53	Junco	1
8	"-24	5:43	F. Eng. Sparrow	2
9	"	10:45	" "	1 Junco made 4 efforts.
10	"	20:18	" "	8 Junco tried twice.
11	"-25	25:30	" "	2
12	"-26	25:10	" "	9 Junco struck at string and seemed ready to jump.
13	"-27	21:00	Junco	5 F. Sparrow not well.
14	"	2:45	"	1 F. Sparrow and Junco were equally good fighters.
15	"	5:05	Female Sparrow	1 on string 4. She drove Junco away.
16	"-28	16:30	Junco	11 F. Spar. 4 efforts. Rivalry.
17	"	10:50	"	26 Rivalry more intense when one tried other followed.
18	"	2:20	Female Sparrow	9 Junco on box as other was on strings. Boiled rice caused Sparrow to try more.
19	"	5:30	" "	7 She did not allow Junco to try. Boiled rice caused Sparrow to try more.
20	"	3:43	" "	1 Junco seemed to beg to be allowed to try. Boiled rice caused Sparrow to try more.
21	"-29	3:05	" "	3 Junco was driven away.
22	"	6:24	" "	6
23	"	1:05	" "	4 For some time she had been using string 4 probably because she was forced there from lack of space above it.
24	"	2:30	" "	7
25	"-30	9:10	" "	18 Junco 14 efforts. No rice in.
26	"	15:	Junco	28 Sparrow 15 efforts. No rice.
27	"-31	:30	"	5 Sparrow on door when it was opened by Junco.
28	"	:55	"	5
29	"	:7	"	1
30	6- 1	:17	"	1
31	"	:28	"	1 On strings twice after door was opened.
32	"	1:55	"	13

TABLE V—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior.
33	6- 1	:57	Junco	5 Sparrow again on door as it swung open.
34	" "	:10	"	2
35	"- 3	3:07	Female Sparrow	1 Junco 9 efforts. As often before, she failed to alight on string.
36	"- 4	:06	Junco	1
37	" "	:32	"	9
38	" "	1:02	"	4
39	" "	:57	"	6
40	"- 5	:05	"	2
41	"- 5	:32	"	9 Did not seem to know yet that the string had to be pushed in.
42	"- 6	:12	"	3
43	" "	:37	"	6 Jumped astride of string 2 as shown in figure 12.
44	" "	:38	"	6
45	"- 7	:20	"	4
46	" "	:30	"	1
47	" "	:25	"	6
48	"- 8	:05	"	1
49	" "	:30	"	1
50	" "	:32	"	7
51	" "	:13	"	1
52	"-10	:17	"	2

In Fig. 13 we find a very interesting condition of things. In this, the twentieth test, the Sparrow, as may be seen, is holding the strategic position, but the jump to string D requires considerable effort in her depleted condition and she is hesitating, making starts, and hesitating again. All the while the Junco with open beak is uttering the most plaintive notes. It is certain that this bird would act at once if given the opportunity. Finally the Junco really showed signs of fighting. To obtain such a situation as this is rarely possible. It would seem that such might be a very favorable condition for the appearance of imitation, and certainly no small amount of suggestion and the "following instinct."

In the twenty-fifth test and those immediately following the boiled rice was omitted in order to see what effect this would have on the efforts of the Sparrow. It is evident that soon the Junco took the lead and with one exception retained it to the close. Both made many efforts. In the twenty-eighth trial the Sparrow was on the door when it swung open. She had jumped there just after the Junco had alighted on the strings. In the thirty-third test she again did the same thing. This series, then, points to rapid learning by the Junco, but to learning the same in kind as that shown by other birds. There

is a place to be hopped upon, but no further does the analysis go. Working on this place brings a pleasurable result, but there is no analysis. The keen rivalry and almost equal fighting ability, the change in the female Sparrow's condition of health and the food more to her liking were particularly suited to bring out some evidence of imitation. This evidence is of the same kind as that in series A and B and other evidence obtained later with the Crows and Orioles. There are some changes in the behavior of both the Junco and the Sparrow, changes from what we are pretty certain would have occurred if the other bird had not been present. These would point to imitation which might be called intelligent. That is, without first giving these birds previous experience with this box we could hardly expect the definite acts called forth in each by the other. There is much proof of the "following instinct."



FIG. 13

The female English Sparrow was ready to jump on the top string. The Junco was standing by uttering a series of plaintive notes. These two birds often fought each other for the position in front of the strings.

Series E.

Tests with the Junco of the Preceding Series, a White-throated Sparrow, a Field Sparrow, a male English Sparrow and a young Song Sparrow.¹

¹It is a duty both pleasantly and gladly performed to be able to acknowledge at this point the courtesy and unreservedness with which Mr. and Mrs. Worthington of the "Worthington Society for the Investigation of Bird Life" placed at the writer's service for purposes of investigation every facility not only as to birds and cages at the aviary

It is well worth our while to note that this series and all that follow in the present paper were made on the birds in out-door wire cages most of which were ten feet square by six high. The conditions were very much more natural if they do not always give such uniform results as may be obtained by working on birds kept in the laboratory. Inside one can control the food supply as well as other conditions so that the birds are more hungry and undisturbed; but it is probably better to sacrifice nice uniformity for naturalness.

This series was really a Memory one for the Junco, the interval being some half a month. During this interval with three Cowbirds this bird was carried several hundred miles from the laboratory in Worcester, Mass., to Shawnee-on-Delaware, Pa., and placed in very different surroundings. The White-throated Sparrow had found its way into a wire cage at Mr. Worthington's Pheasantry, and was captured and placed in this cage. Its size and disposition, and ability to fight account for its early successes and for the long times even when the Junco opened the door. The White-throated Sparrow would not allow any of the birds to come near the box. This was not true for the first of the Junco's trials and it will be seen from the table that they were good. The short time of the Junco in the seventh trial, its first success, indicates that previous to this the White-throated Sparrow and the strange surroundings served to keep it away from the box.

The White-throated Sparrow, if we make an exception of her earlier trials, may be said to have opened the door in the manner used by the Junco. But she did not long continue this. She jumped up on the post rather than on the string. At times her efforts were as many as seventy-two and ninety-two, and largely for the reason that her efforts had to do with the post and not with the string, Fig. 14. This method was used habitually, and points to the kind of associations of which this bird was capable.

For the 129th test the food was shifted to the left side of the door, and in the next to the left rear corner. This was done with many of the birds in order to see whether it would lead them away from the strings, and cause them to work on another part of the box which was very similar in appearance to that where the strings were placed first. This change had no effect at this time, but after this bird and the Song Sparrow were transferred to another cage, such a change in the place of the food did have quite a disturbing effect. See tests 159, 160 and

but also in many other respects. My acknowledgments are gratefully made to Mr. Chas. W. Miller, Director of the Society, and to Mr. W. H. Montgomery for carrying on experiments during the writer's enforced absence, as well as for many other services.

161. In fact it was this change which introduced sufficient delay to make it possible for the young Song Sparrow to open the door. She did not, so far as could be seen, do so in an imitative way. Later there were a few movements which were similar to those of the White-throated Sparrow, but they did not have to do with the right part of the box.



FIG. 14

This shows the method which the White-throated Sparrow used to open the door. She often was compelled to make many efforts because she alighted on the post rather than the outer end of String B,

TABLE VI

The Junco of series D, a Field Sparrow, a White-throated Sparrow and a male English Sparrow. Results of tests with the food-box.

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior
1	6-24	10:30	Failed	
2	"	33:40	{ White-throated Sparrow }	1 White-throated Sparrow is much the largest.
3	"	5:10	Field Sparrow	7
4	"	1:30	White-throated	1
5	"	1:50	Field Sparrow	1
6	"-25	1:35	White-throated	1
7	"	:55	Junco	1
11	"-26	7:00	"	19 Strings damp. Early morning tests.
12	"	2:12	"	41 " "
16	"	4:26	"	9 Raining.
17	"	:20	"	1
18	"	5:21	"	27 White-throated not allow Junco to come near for some time.
19	"-27	:14	"	2
43	7- 4	:10	"	1
44	"- 5	4:10	White-throated	2

TABLE VI—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior.
45	7- 5	1:50	Field Sparrow	1
46	" "	7:16	White-throated	1
47	" 6	:40	"	1
48	" 7	1:10	"	2
49	" "	1:35	Field Sparrow	1
50	" 8	3:10	White-throated	1
51	" "	3:50	"	1
61	" -13	7:35	"	9 Junco 7 efforts.
62	" "	4:00	Junco	4
63	" "	10:40	White-throated	6 Junco 26 efforts.
64	" "	3:05	Junco	12
65	" -14	19:35	Failed	Junco 21, White-throated 50, Field Sparrow 1.
66	" "	1:20	White-throated	1
67	" "	:25	Junco	1
70	" -15	:48	"	2 Male Eng. Sparrow in the cage. Junco on string with door open.
71	" "	2:13	White-throated	1
72	" "	5:50	Junco	1 Male Sparrow tried twice.
78	" -18	16:00	Failed	White-throated 22 efforts, Junco 25, Field Sparrow 1.
79	" "	8:00	"	Raining.
80	" "	7:40	English Sparrow	White-throated 16 efforts, Junco 11 efforts.
81	" "	2:08	Junco	1
82	" -19	:38	White-throated	1
83	" "	:48	"	1
84	" "	2:11	"	2 Struck once with beak.
85	" "	2:10	Junco	2 Junco mistook left end for front.
86	" 20	1:43	White-throated	12 efforts.
89	" "	:45	Junco	1
90	" 22	9:30	White-throated	72
102	" 25	6:38	Junco	1 White-throated 4 efforts.
103	" "	4:05	White-throated	3 Young Song Sparrow 10.
112	" 29	1:30	"	10 All mere passes at string. Really on post. See Fig. 14.
129	8- 3	:15	"	1 Food in left front corner. No effect.
130	" "	2:18	"	1 Food in left rear corner. No effect.
131	" 4	1:10	"	1 White-throated and Song Sparrow in a different cage.
132	" 5	:45	"	1
133	" "	1:46	"	1
134	" "	1:20	Song Sparrow	3
135	" "	2:19	White-throated	2 Song Sparrow 2.
136	" 6	:35	"	1
159	" -13	2:22	"	6 Food to left of door caused some of the efforts to be on the left side.
160	" "	1:30	"	3

TABLE VI—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior.
161	8-14	1:38	{ White-throated Sparrow }	15 Food in left rear corner. Greater confusion.
162	"-"	1:13	Song Sparrow	3 Not in imitating way.
163	"-"	2:16	"	3 Song Sparrow 7.
164	"-15	3:55	White-throated	1
165	"-"	1:55	Song Sparrow	4 Song Sparrow 1.
166	"-16	:32	White-throated	1
167	"-"	1:52	"	2
168	"-23	:17	Song Sparrow	1
169	"-"	1:04	White-throated	1 Song Sparrow 1.
170	"-25	6:38	"	34 New box. Must jump higher for string B.
171	"-"	7:25	"	35
172	"-26	1:38	"	10
173	"-"	2:45	Song Sparrow	2 One effort by Song Sparrow might be called an imitation of White-throated.
174	"-"	:14	"	1
175	"-27	:30	"	2 One as in 173.
176	"-"	1:50	"	7 Two as in last test.
177	"-28	1:22	"	3 Successful by flying from a distance.
178	"-"	1:20	"	2
179	"-29	:11	"	1
180	"-"	1:55	"	1
181	"-30	:47	"	1
182	"-"	2:12	White-throated	17
183	"-31	2:50	"	5 Song Sparrow 3.
184	"-"	1:16	Song Sparrow	1
185	9- 1	5:32	White-throated	49 Song Sparrow 11. Many times the White-throated between strings A and B as the Bluebird.

In the 55th test the Junco mistook the end of the box for the front. I foresaw by her behavior that such was likely to happen and there was no error in the observation.

The new box used after the 169th test was a source of trouble for the White-throated Sparrow. String B was an inch or so higher than it was on the old box. She should now have jumped higher, but she did not learn readily to do so. This again was favorable to the Song Sparrow, who now opened the box until toward the close of the series.

The present series is not satisfactory from the standpoint of imitation. There are some signs of it, but they are inconclusive and do not satisfy the criterion which is adhered to in this paper. These results are interesting from the fact that new birds are tested and found to learn in a typical manner. If not so rapidly it may be due to the location of the cage and the interference of the White-throated Sparrow. She did not

learn rapidly; that is, the results for a time at least do not indicate it because of her indirect method. This method shows that she had no comprehension of the relations involved between parts of the box. The Junco probably forgot very little during the interval of half a month. It simply required a few trials to overcome the effects of the changed conditions. The effect of changed conditions is seen in the behavior of the White-throated Sparrow on the transfer of the food-pan in the strange cage.

Series F.

Tests with the Food-box on a Field Sparrow, a Junco and the male English Sparrow of the previous series after the White-throated Sparrow and Song Sparrow had been Removed.

These tests are open to the same disturbances which affected the last series. This cage was small, near the door into the building which opens into all the cages and nearest to the rear entrance of the main building. It follows that there were many disturbances.

It is to be noted that the Junco, the same as figured in series D and E, opened the door but three times during this series. The Field Sparrow seemed to get hungrier than the others. This and its greater tameness may explain why it was the bird with which we must deal in the present series.

In the 5th test the little Field Sparrow used a method similar to that used by many of the English Sparrows. This was to fly out from the upper front edge of the box, turn, and alight on one of the higher strings. Often alighting on the string did not open the door, but when she started to fly away the additional jerk she gave the string was sufficient to lift the latch. Frequently, however, this did not open the door. Very early she formed the habit of jerking the string by many flaps of the wings.

Her slight weight is the one chief cause of the fact that the times remain long and the efforts many. In general she learned as the others. As with many of my birds, though perhaps to a greater degree, she illustrates that fixity of the order of movements which they follow in the process of doing the thing learned. Her order was to fly first to a branch at the top of the cage, then to the water faucet, thence to the left end on top of the box, then to the right corner and thence off on the strings. Her conformity to this order was not without exceptions, but it was certainly striking.

TABLE VII

Really a continuation of the previous table but with only one Field Sparrow, the Junco of Series D, and the male English Sparrow in the cage.

Trial	Date	Time	Bird opening box	Efforts of bird opening the box
1	8- 4	8:47	Field Sparrow	8 This bird was very much smaller than the others.
2	" "	2: 5	"	3
3	"- 5	9:50	"	10
4	" "	4:42	Junco	2
5	"- 6	4:45	Field Sparrow	7 Flew out from top of box turned and alighted on string.
6	" "	2:35	"	4
7	" "	3:35	"	2
8	" "	5:10	"	3
9	"- 7	5:40	"	15 She was not heavy enough.
10	" "	9:40	"	1
11	"- 8	5:59	"	2
12	" "	1:12	"	2
13	"- 9	4:37	"	12
14	" "	:30	"	1
15	" "	2:28	"	4
16	"-10	4:16	"	2
17	" "	4:38	"	1
18	" "	1:50	"	6
19	" "	9:10	Junco	1
20	"-11	1:10	Field Sparrow	3
21	" "	1:06	"	4
22	"-12	1:29	"	2
23	" "	:50	"	3
24	"-13	:53	"	4
25	" "	:33	"	3
26	"-14	:14	"	2
27	" "	1:28	"	4
28	" "	:42	"	3
29	"-15	:12	"	1
30	" "	1:	"	2
31	"-16	:50	"	2
32	" "	1:40	"	1
33	"-17	:13	"	5
34	"-23	2:23	"	3
35	"-25	1:	"	4
36	"-26	:40	"	6
37	" "	:22	"	1
38	"-28	2:10	"	3
39	"-30	:30	"	2
40	" "	:17	Junco	1
41	"-31	1:48	Field Sparrow	2
42	9- 1	:19	"	

Series G.

Result of Tests with the Food-box on a pair of Bluebirds and White-crowned Sparrows; also two of each of the following named species,—Juncos, and Song, Tree, and Fox Sparrows.

There were probably for the best results too many birds in

this cage. There is apt to be too much fighting and it is impossible to keep a satisfactory record of all that is done by the different birds. The chief cause for having so many was that all the other cages were occupied by birds engaged in mating or in rearing young. Indeed it was later learned that these Bluebirds were so engaged. This fact may partly explain why they came to be masters of the situation in this instance.

It is interesting to note that it is the best fighters here, as in a series earlier described, which first open the box. However, this does not mean that they continue to hold first place throughout the series.

The Bluebirds exhibited that characteristic timidity which may be seen in a little observation out of doors. They never attacked the other birds and strictly minded their own business. Many times they were compelled to beat a hasty retreat by the Tree, White-crowned, or Song Sparrows.

If what one may observe in a wire cage of this size (ten feet square by six feet high) of the insect-catching habits of these birds is any criterion, the Bluebirds far outclass any of the others. They gave abundant evidence of being able to see small insects when at least some six to eight feet distant and, of course, lost no time going after them.

From the accompanying table it may be seen that there is much alternation at first. The Bluebirds are not much in evidence until the 6th test when the male opened the door, the female being successful on the 7th test. This looked like imitation and yet as she had no previous method it cannot be said that she was following the example set by the male in what she did. For the present it is probably best to regard their behavior as similar responses to the same situation, with a strong tendency in these Bluebirds to follow each other rather than any of the birds of the other species. The Bluebirds were the rightful owners of this cage. The other birds were turned in only a few days before the tests were begun.

TABLE VIII

Results of tests with Food-box on a pair of Bluebirds and White-crowned Sparrows; also two each of Tree, Song and Fox Sparrows; also two Juncos.

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior
1	6-15	4:50	Tree Sparrow	11 Stepped on string. Tree Sparrow was a good fighter.
2	" "	5:45	{ White-crowned Sparrow }	4 White-crowned Sparrow stepped on string was a good fighter.
3	" "	3:	"	3
4	" "	20:45	Tree Sparrow	2

TABLE VIII—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by Bird opening Box and other behavior.
5	6-15	7:25	Tree Sparrow	4
6	"	3:	Male Bluebird	2 By alighting on strings.
7	"	1:25	Female "	2 " " "
8	"	:40	Tree Sparrow	2
9	"	:25	"	1
10	"-16	1:05	Female Bluebird	1
11	"	5:55	Male "	2
12	"	5:25	" "	5
13	"	:20	Female "	1
14	"	9:05	Tree Sparrow	1 Female Bluebird many efforts.
15	"	7:20	"	1
16	"	1:05	Fox Sparrow	1
17	"	4:40	M. White-crowned	1
18	"-17	8:	"	1
19	"	1:30	Song Sparrow	3
20	"	4:20	" "	2
21	"	2:35	Male Bluebird	2
22	"	4:55	Fox Sparrow	1
23	"	2:38	Male Bluebird	2
24	"	9:35	Tree Sparrow	2
25	"	1:	M. White-crowned	1
26	"	12:55	Male Bluebird	4
27	"	2:45	"	2
28	"	2:05	F. White-crowned	1
29	"	2:40	Tree Sparrow	5
30	"-18	17:47	Male Bluebird	1 Early morn'g tests, strings damp, and pull hard.
31	"-18	23:10	Tree Sparrow	8
32	"-19	:20	" "	
33	"	1:	Junco	
34	"	:40	Male Bluebird	
35	"	:40	Female "	
36	"	:50	Male "	
37	"-20	:50	" "	
38	"	1:35	Fox Sparrow	
39	"	:05	Male Bluebird	
40	"	2:15	Tree Sparrow	
41	"-21	8:35	Male Bluebird	
42	"	2:25	White-crowned	
43	"-22	1:30	Tree Sparrow	
44	"	1:15	Male Bluebird	
45	"	18:30	Fox Sparrow	
46	"-23	2:10	Tree "	5
47	"-24	:55	Male Bluebird	1
48	"	3:30	Female "	1 From perch to string as male Bluebird often did.
62	"-26	6:11	Male "	1
63	"	2:27	Song Sparrow	1
64	"	6:35	Male Bluebird	4
141	7-17	:13	" "	3

Much the same succession followed in the 11th test for the male, who opened the door in the same manner as the female had

just done. In the 35th, the 48th and the 55th tests she opened the door in much the same manner as both had done previously. According to my notes the correspondence between the behavior of these two birds is close, but there is little proof that one changes its behavior because of the example set by the other. Fig. 15.

At about the 47th test the male Bluebird began regularly to open the door. The table is not carried farther than the 64th test for the reason that nothing except a lowering of the time and number of efforts occurs. There is little necessity then for a tabular statement of the remainder of the 141 tests. It should be added that the number of efforts by the male Bluebird is often greater than would be expected. An explanation for this may be found when we see what occurred in the Memory series which were made after one month had elapsed.

Memory Tests

For these tests the Bluebirds were alone and in a smaller cage. They were placed here sometime prior to the beginning of this series. From any of the results, or an average of the entire number, it may be seen that they have by no means lost all the training due to their earlier experiences with this box. Their average time is perhaps three or four times what it was at the close of the initial series. Barring disturbing factors in both the initial and first part of the memory series the number of efforts required is about the same. It is probable

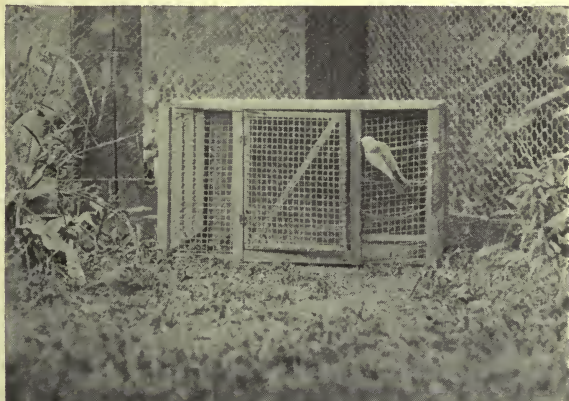


FIG. 15

The male Bluebird in characteristic position to open the door. A white-crowned Sparrow in front and to the left.

that the changed conditions cause the time to be longer. In fact it was noted at the time that such was the case.

Aside from showing how well they remember, this memory series shows some very interesting happenings. How they remember is even more significant. For the 5th test I inadvertently placed the food-pan behind the door instead of just inside the wire from the niche where the strings were placed. The male Bluebird at once made six trials on the door where there were no strings. I then interrupted long enough to place the food-pan in its old place and he immediately alighted on the wire between the strings. Are we then to infer that he has been primarily alighting in a certain place and rather as an accompaniment of this place association he has through no definite intent of his been pushing the strings in or down and opening the door? The greater number of efforts than we should expect in the last part of the initial test series belongs with this sort of association and really should have prepared us for the fact just mentioned.

TABLE IX

Memory tests for Bluebirds. One month interval.

Trial	Date	Time	Bird opening Box	Efforts of Bird opening Box and other behavior
1	8-17	:30	Male Bluebird	1
2	" "	:25	" "	1
3	"-18	:20	" "	1
4	"-23	:20	" "	1
5	"-24		" "	6 Food-pan placed just behind door. Male Bluebird made 5 efforts. Food pan then placed behind strings. One effort there opened door.
6	"-25	:13	" "	1
7	" "	:46	" "	4 A new box with strings A and B slightly wider apart. Many times to the wire without touching strings.
8	"-26	1:12	" "	7
9	" "	:25	" "	4
10	" "	:30	" "	3
11	"-27	1:08	Female "	1
12	" "	:55	Male "	7
13	"-28	:30	" "	5
14	" "	1:13	" "	5
15	"-29	:48	" "	4
16	" "	:28	" "	6
17	"-30	1:13	" "	6
18	" "	:16	" "	1
19	"-31	:25	" "	1
20	" "	:30	" "	3
21	9- 1	:20	" "	1

In conjunction with the next observation to be related and many other facts concerning the kinds of error made by birds of other species, it would seem possible to throw some additional light on the extent to which these birds really analyze the situation which confronts them.

For the sixth test of this Memory series another box to be opened was used. This new box had to be used with the Bluebirds, Field Sparrows, and White-throated Sparrow, in order that I might change the fastening on the old box and use it with the Baltimore Orioles and Blue Jays. The new box was identical with the old except that a slightly wider space was left between strings A and B.

The number of efforts required for the male Bluebird to open the box at once increased. It was easily observed that he alighted on the wire between strings A and B. Ostensibly his trials were more numerous for the reason that there was now greater space here and he could hop on the wire without striking the strings. If this is true then what is good from the standpoint of Memory alone need not signify much when we inquire as to the degree of analysis of which this bird is capable. We get a suggestion from this of how lacking in real analysis any act of bare recollection may be. These strings have not meant and never could mean to him at all what they mean to human consciousness. It was not for him the strings to pull, but a place to alight; more correctly perhaps he just alighted. Even saying no more than that it was a place to alight is putting it too anthropomorphically. True the bird is intelligent, but his consciousness must be far more simple, vague and unanalytic than our own.

Imitation is not clearly shown in this Memory series. The female does at times make trials at the same time with the male or follow him closely. But as in so many other series of tests, the writer feels that his criterion has not been met in a satisfactory way.

Series H.

Tests with Food-box on a Pair of Blue Jays.

The results obtained from a long series of tests with these birds is doubly interesting for the reason that this pair of Jays was reared by hand. They were very tame, or soon became so. Often they alighted on my back while I was placing the box in the cage. If at all hungry the male had to be carefully watched or he would alight on top of the box and take food from the pan as the box was carried into the cage.

These Jays, particularly the male, were very musical. It is said that much of it comes from the imitation of musical instruments like the mandolin, guitar and violin, all which they

have had opportunity to hear. However uncertain this may be, I have never heard such notes by Jays in their natural haunts. I doubt if many other birds can surpass such low, sweet, metallic notes which they were wont to put together in the most pleasing way.

The Jay, like the Crow and English Sparrow, is popularly considered as a very intelligent bird. As with the Crow, it was to be expected that they would use the beak on the strings, Fig. 16. This they did throughout, making only a few attempts to open with the claws or hop up on the box as the smaller birds do most frequently.

The beginning tests, as may be noted, if the reader will refer to the table below, are of the same general kind which have been found for all smaller birds. This is true, particularly of all those which are compelled to open the door without any example by another bird which may serve as a model to be imitated, or what is probably more often the case, as only of general suggestive value which calls into play nothing more than what may be designated as the following instinct.

It should be said that because of lack of time these Jays were not allowed to eat from the box previous to the first tests and thus get accustomed to obtaining their food from it. This, of course, might have affected the first few results, but only these.



FIG. 16

The male Blue Jay is in the act of opening the door by pulling the knot on String B. In test 110 (Table X), though the strings had been changed to the left of the door, this bird persisted in opening the door by reaching through the wire and pulling up the latch.

TABLE X

Test with Food-box on Blue Jays.

Trial	Date	Time	Bird opening Box	Efforts of Bird opening Box and other behavior
1	7-15	30:	Failed	
2	"-"	1:35	Male Jay	
3	"-"	1:05	"	I Chased away female
4	"-"	:20	"	
5	"-16	20:22	"	32 Female many efforts also.
6	"-"	1:35	"	10
7	"-"	2:13	"	29
8	"-"	7:08	"	51 He did not allow her to come near the box.
9	"-"	3:37	"	5
10	"-17	15:	Failed	Not very hungry.
11	"-18	15:	"	8 efforts by female.
12	"-"	4:25	Female Jay	I In same way as male. The only sign of imitation in the series.
13	"-"	:35	Male Jay	I
14	"-"	1:20	"	I
15	"-"	2:22	"	11 Most of these incorrectly placed.
16	"-19	1:32	"	2
17	"-"	1:13	"	3
18	"-"	:48	"	I
19	"-"	3:10	"	7
20	"-"	1:08	"	6
21	"-"	3:13	"	3
22	"-20	1:30	"	6
23	"-"	:30	"	I
24	"-"	2:25	"	8
25	"-"	:47	"	3
26	"-21	:20	"	I
27	"-"	:47	"	I
28	"-"	:18	"	I
29	"-"	:12	"	I
30	"-"	:17	"	2
31	"-22	:16	"	I
32	"-"	:17	"	I
33	"-"	:25	"	I
34	"-"	:24	"	I
35	"-"	:35	"	4
36	"-23	:12	"	I
37	"-"	:18	"	I
38	"-"	:20	"	I
39	"-"	:18	"	I
40	"-"	:08	"	I
41	"-"	:27	"	2
42	"-"	:08	"	I Returned twice to pull the string five times with door already open.
43	"-24	:12	"	I
112	8-25	:05	"	11 Strings to left of door but opened through wire at old place. Five pulls after door opened.

TABLE X—*Continued*

Trial	Date	Time	Bird opening Box	Efforts of Bird opening Box and other behavior.
113	8-26	:20	Male Jay	2
114	" "	:10	"	2 Two efforts on the strings to the left of the door but finally used old method in all the remaining trials.
115	" "	:02	"	1
116	" "	1:08	"	9
117	"-28	:10	"	1
125	9- 1	:25	"	6

Some of the extremely long times early in this series may also be due to the same fact that had to be kept in mind with the Crows. The Jays also store away their over-supply of food and they may not have been as hungry as one would infer from the period of time since they were last fed.

Knowing the use which the Blue Jay makes of his beak in the opening of nuts, etc., it was not surprising that he should use the same method as the Redheaded Woodpecker and the Crows were found to do in such a vigorous and persistent manner.

The female Jay showed very little tendency to imitate. She opened the door but once and this in the 13th test, though my notes several times make record of the fact that she might have done so if opportunity were all that was needed. It should be said, however, that during the first tests she really had little opportunity. The male was certainly a tyrant in this family. The female, as a rule, never entered the box to eat until the male had satisfied his hunger and left. Not being allowed near the box at the start, she formed the habit of staying away, and this is directly inimical to imitation in later trials. She followed him to the box often, however, as indeed all the birds have done with each other.

In the 43rd trial the male for some reason left the box after taking only a few bits of food. He soon returned and, with the door already open, pulled the string three times. He then went to the top of the box, but did the same thing twice more on his next approach. In the 112th trial he again repeated the same action under exactly similar conditions five times. He did, therefore, what the other birds, most of them, have been seen to do.

By the 60th trial this Jay has probably learned to open this box as readily and easily as he ever will. The table, therefore, omits the results of tests 44 to 58 and 60 to 109 inclusive.

For the 110th test the strings were transferred to the left of the door. The Jay struck twice on place 1, then hopped up on the wire at the same place, then twice on the strings in

their new position, place 2, and lastly seven in place 1 where he succeeded by reaching through the wire in the old place and pulling up the latch. But once during the remainder of fifteen trials did he make use of the strings. It is evident from the number of efforts during these final tests that it is the place which is firmly fixed, or associated, for he will make a great many efforts through the wire on the thread or projecting end of the latch rather than work on the strings. These, too, have only been changed to the left of the door and they are very conspicuous.

We get, then, from this series proof of the ability of the Blue Jay to learn. This he may do somewhat more rapidly than some other birds, but really in very much the same way. As proof of imitation we get very little as compared with some of the other groups of birds. But in these instances there would seem to be that which one may sometimes get from negative results. This male Jay had no rival, he was not as cautious as the Old Crow and the male English Sparrow, and there were no long intervals of waiting as with the latter and the Cowbird.

Series I.

Tests with Food-box on two male Baltimore Orioles, three Cowbirds,—two males and one female,—and two young English Sparrows.

At the beginning of the present series there was but one male Oriole present. The young English Sparrows, though they were partly reared by hand, were so wild that they did not approach the food-box at all in the early tests. It is true they could not be expected to cope successfully with the mature birds of the larger species, and yet if at all hungry they could at times take care of themselves surprisingly well.

During the first four trials, and once following this, one of the male Cowbirds opened the door. In this series these initial times are relatively short. This is probably due to the fact that the strings were rather easily worked and the birds rather hungrier than in some of the other series. The Cowbird's method was one somewhat peculiar to this bird, though not entirely so. He lifted one foot and clawed the lower strings. In the second test he must first fight off the Oriole. Close observation showed, that the latter might be outclassed as a fighter, but would not necessarily be driven off. He would open his mouth, express his emotional state in rapidly succeeding cries, fall back on his haunches, and stand his ground. In the 4th trial the same Cowbird changed his method and used his beak on the string.

In the 5th test the Oriole took his turn, but in an obviously

accidental manner. It is, of course, understood that all these first successes are accidental. In this case it will be easily seen it was if possible more so. In flying to the top on the front side of the box the Oriole slipped off and struck the top-most string. He succeeded in the next trial by using his foot on the lower string. The 7th trial required an inordinately long time and his final method is far removed from the simplest way in which the door may be opened. What he did was to put the beak through the wire and pull the black threads which run vertically from the latch in order that they may be attached to the outside strings. A most indirect method, one which

TABLE XI

Tests with Food-box on two male Baltimore Orioles, three Cowbirds and two young English Sparrows.

Trial	Date	Time	Bird opening Box	Efforts by bird opening Box and other behavior
1	7-21	:40	Male Cowbird	By stepping on lowest string.
2	"	3:55	" "	By stepping on lowest string. Oriole 1 must be driven away.
3	"	6:15	" "	By stepping on lowest string. Oriole 1 must be driven away.
4	"-22	5:28	" "	7 Pecked at each end of latch. Cowbirds afraid. One eye turned toward sky.
5	"	5:20	Oriole 1	Struck top string in going to top of box. Young English Sparrows very wild though reared by hand.
6	"	1:50	"	By foot on string.
7	"	8:38	"	By beak through wire mesh on threads. Oriole 1 chased Cowbird away.
10	"-23	23:58	"	24 Cowbird chased away nine times.
11	"	5: 3	"	14 Cowbirds not down.
12	"	4:15	"	14 Oriole opened by pulling knot on lowest string.
13	"	1:55	"	1
14	"-24	3:43	Cowbird	3 In same way as he did in 4 above. Oriole 1 made two efforts through wire at threads.
15	"	4:40	Oriole 1	7 Oriole 1 opened through wire.
18	"	1:28	"	10 9 of which were on threads with beak
30	"-29	1:	"	13 Oriole opened by working string B on outside.
31	"	:20	"	1 Returned to old method.

TABLE XI—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by bird opening Box and other behavior.
40	8- 1	:47	Oriole 1	41 Oriole finally opened by striking outer string B.
41	" "	:52	"	4 Beak on end of latch.
48	" 4	:57	"	3 A second male Oriole in cage. He was clearly master. Made many efforts.
49	" 5	:23	"	3
50	" "	:12	"	1
51	" "	1:10	"	2 Oriole 2 much in the way. O. 2 after O. 1 has entered made some 10 efforts very like those of O. 1.
52	" 6	:28	"	3
53	" "	1:25	"	3 O. 2 entered first. O. 1 pulled strings 4 times with door open and O. 2 inside.
54	" "	:27	"	1 O. 1 pulled strings 3 times with door open.
55	" "	:15	Oriole 2	By standing on string 1 and striking on 2.
56	" 7	:47	"	By striking. O. 1 driven away just as he was opening door.
57	" "	1:	" 1	In his old way.
58	" "	2:	" 2	By flying on string D. O. 1 driven away.
59	" 7	:51	" 2	By flying up on string D.
63	" 8	:48	" 2	By accident. O. 2 slipped off post and struck string C.
64	" "	:12	" 1	In his old way. O. 2 followed up the accident of last time and missed.
65	" 9	:15	" 1	In his old way Same as before.
66	" "	:41	" 2	5 All at strings C and D.
67	" 10	2:02	" 2	6 Many of these efforts were made by following up the accident.
68	" "	:04	" 2	1 on string D.
69	" "	:33	" 2	5 went to post, thence to string D.
72	" 11	4:12	" 1	O. 1, 15 trials. O. 2 8. Something wrong with box. O. 1 opened in old way but went to post once as O. 2 did often.
73	" "	:27	" 1	O. 2 was clinging to door when it opened.
74	" "	3:45	" 2	6 O. 2 to post once and on door 5 times. O. 2 to string C from door.

TABLE XI—*Continued*

Trial	Date	Time	Bird opening Box	Efforts by bird opening box and other behavior.
76	8-12	:23	Oriole 2	5 efforts, 4 on door.
77	"-"	:03	" 2	From ground to string C.
83	"-15	:05	" 2	O. 2 from door to string C.
89	"-25	:20	" 1	Strings changed. Opened in old way. O. 1 followed O. 2 on door.
90	"-25	:30	" 1	O. 1 in old way. O. 1 again follows O. 2 to where string should be.
91	"-26	:15	" 1	O. 2 made many trials.
92	"-"	:15	" 1	O. 2 made many trials. O. 1 made two efforts like O. 2.
93	"-"	:03	" 1	2 efforts in old way.
94	"-"	:15	" 1	3 efforts in old way.
95	"-"	:15	" 1	15 in old way.
101	"-29	:05	" 2	1 on string C.
102	"-"	:25	" 1	3 O. 2 made 1 effort on left of door.
103	"-30	:28	" 1	11 efforts. O. 2 made 1 effort on left of door.
104	"-30	:10	" 1	5 Some tendency in O. 2 to use bill.
105	"-31	:05	" 1	2 O. 2 to left of door.
106	"-"	:07	" 1	1 O. 1 showed tendency to follow O. 2.
109	9- 1	:08	" 1	1

we shall later find the Crows using, and a method that may give the experimenter both positive and negative results for which he has not reckoned.

By the 9th trial the Oriole constantly chased the Cowbirds away from the box. On the 14th test the same male Cowbird opened the door with the beak on the end of the latch by reaching through the wire mesh. As he did this but once it can hardly be counted as a good example of imitation.

For the 48th and succeeding trials the second male Oriole was placed in the cage. It was evident at once that he was more pugnacious than Oriole 1. There was no doubt that he would be ruler of this cage in a short time. He made efforts to get into the food-box during this his first trial. Three tests later, after Oriole 1 had opened the door in his usual manner (by reaching the beak through the wire mesh and pulling threads), the behavior of Oriole 2 was most significant. He made many of the same kind of efforts as Oriole 1 had just made but the latter was inside eating and the door was open.

Oriole 1 continued to open the door for three more trials. In the 53rd Oriole 2 entered first and Oriole 1 pulled the threads fourteen times with the door standing open. In the following

test he did the same thing four times under similar conditions.

There now began that period of alternation which has happened in previous series in which first one then the other was successful. The first method of Oriole 2 was to stand on the lower string and strike the one just above. Several times Oriole 1 was chased away just as he was on the point of pulling the threads. In the following trials Oriole 2 was successful by flying up on strings near the top. It is evident that if he did imitate it was not for long. Flying up and pushing the top string is certainly not an imitation of Oriole 1 in his reaching through and pulling the threads. The number of efforts required of Oriole 2 at this stage would indicate that he had not yet singled out the single part and only that which must be worked.

The 62d test gave interesting proof of the statement just made. Oriole 2 alighted on the post just above the outer end of string C. In an apparently accidental manner he slipped from the post and struck this string and the door opened. Would he use this method in the following trials? I was watching for him with camera focused and the accompanying figure, 17, was the result. His erroneous association and the efforts which he made to operate the strings by merely alighting on this post was what allowed Oriole 1 to step in and be successful during the following two trials. This, too, in face of the fact that Oriole 2 was the first at the box. In the later trials the latter would often go to the post and thence up to the top string.

Usually in learning tests with animals it is unfortunate if the fastenings of the food-box fail to work as easily or in the same way as they have in the past. Yet as we shall see directly for these Orioles and later for the Crows, such an accident in tests on imitation is really a favorable occurrence. Reference to Table XI will show that the time is greatly lengthened for the 70th and 72d tests. It was evident that there was something wrong with the box. Oriole 1 was successful only after fifteen attempts, all save one of these being on the threads. Oriole 2 made eight attempts at hopping on the post and Oriole 1 made one such attempt. Apparently in imitation of Oriole 2, and in a very sudden and impulsive manner. In the 71st trial Oriole 1 opened while at the same time Oriole 2 was hanging to the door. What was the effect of this in the next trial? The length of time in this next trial is due partly to the fact that Oriole 2 must hop on the door five times. In fact he left the door to go directly to string C, Fig. 18. Again something entirely unnecessary becomes to this bird an important link in the association series because his hopping on

the door was simultaneous with the pulling of the thread by Oriole 1.

For the 87th test the strings were removed from the right and placed in the niche to the left of the door, place 2, Fig. 19. Oriole 2 was on the door and went thence to where string C used to be. Oriole 1 opened in his usual way by pulling the threads. In changing the outside strings to the left of the door I had, of course, intended to modify the box for Oriole 2. At any rate if we failed to get results on the readiness with which Oriole 2 could do as the female English Sparrow did in series A, we did get some slight indications of imitation of Oriole 2 by Oriole 1. It was very similar to the result obtained a little earlier in this series. In the following test there was likewise a single indication of imitation.

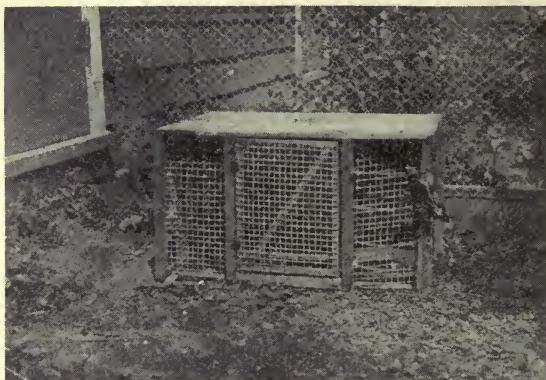


FIG. 17

Male Baltimore Oriole 2. He, during an earlier trial, flew to this post, slipped off, struck String C and opened the door. He is shown here continuing this wholly unnecessary action.

For the remainder of this series Oriole 1 continued to open the door. Oriole 2 to all appearances saw him, but as they began to moult badly I did not improve the mechanism as I did later for the Crows in order to compel just one method of gaining entrance to the box. It is probable that these Orioles, had the tests been continued, would have given much better evidence of the power of imitation.

Great interest naturally attaches itself to these tests for the reason that at least one popular writer on the Baltimore Oriole has found it capable of very remarkable performances. It is said to resort to measures distinctive of an adult human being in arranging supports for its nests and in the tying of knots in

strings used for such supports. Indeed many human beings would be incapable of what was reported of this pair of Golden Robins.

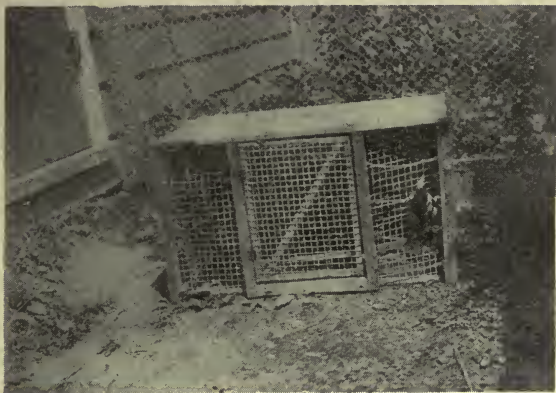


FIG. 18

Male Baltimore Oriole 2 as he came to open the door after many other attempts.

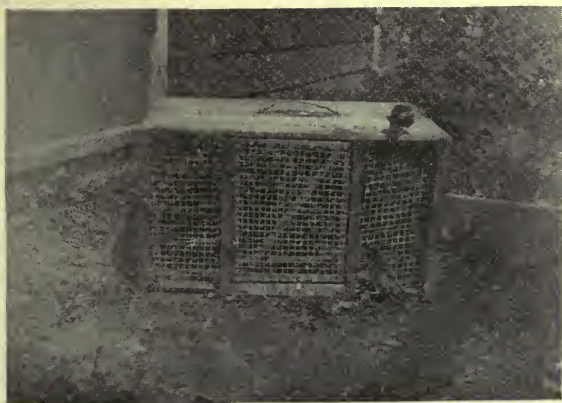


FIG. 19

The strings are to the left of the door, Place 2. Male Baltimore Oriole 2, instead of working the strings in the new position, tried the old place, failed, and was here watching Oriole 1 reach through the wire as the latter was accustomed to do earlier.

The writer has been much interested in the many abortive attempts of pairs of Baltimore Orioles to build their nest in the

cages of the aviary where the above tests were made. When supplied with much material which they may naturally be expected to use, all their nests are imperfect, some much more so than others. It may be seen that in all of them an attempt is made to adhere pretty closely to the nest so typical of this species so many of which are found on the slender elm branches over our streets and sidewalks. Not a single observation on many such imperfect nests for a number of years would go far to corroborate the above remarkable findings with respect to nest building. Such astonishing results would for their satisfactory explanation require reasoning in these Orioles. The present pertinent query is,—Did the two male Orioles who were submitted to all the above tests show any signs of any mental power above that of intelligence or the "trial and success" method of learning? It should be said in this connection that to infer from an isolated single instance that any animal reasons or does not reason is a method hardly to be tolerated in our present science of Animal Behavior. In fact, the study of human reason requires a series of tests with the same problem or situation from day to day.

In the above tests it is clearly evident that each bird, the male Cowbird, then Oriole 1 and lastly Oriole 2, first learned by accident how to operate the fastenings and in his own way. Even after Oriole 2 had done the right thing for a number of times a little accident led him astray. In so far as there is imitation it is shown by Oriole 1 when he copied certain actions of Oriole 2 which were never calculated to bring any results. They were errors on the part of the latter. As such they were imitated very suddenly and impulsively. Such imitation could hardly be rational or reflective. Intelligent it may be. Again, if there is any reasoning in all this ought not Oriole 2 have opened the door when the strings were changed from the right to the left side? He had worked the strings on the right side but twenty-six times all told. He certainly could not have formed so fixed a habit that all his power to reason, if he had any, was made as if it were not by habit. Truly these Orioles fall into the same class with all other animals which have been subjected to careful continuous investigation which exhibits to us the origin and development of the learning process. They learn gradually to perform just that act which rather directly brings the pleasurable result. The writer does not know of any series of experiments which are better calculated than these with the Orioles to illustrate the direct following up of acts which by merest accidents brought the bird to the food. Such, it seems to me, is the correct interpretation to place upon the results embodied in the present section of this paper.

Series J.

Intelligence and Imitation in Crows as Determined by Tests in Opening the Food-box.

The old Crow, Jim, with which my first work on Crows began had been in possession of Mr. Worthington for some five or six years. His wings had been clipped and most of the time he was to be found in some part of the large lawn. For a year or more he had been confined to a large wire cage. He was very wary. Even an acquaintance must exercise care in pulling Jim's beak or scratching his head for at such times he was apt to use his bill with painful results.

It should be added that the sex of this bird as well as that of the young crows was not known. If they are referred to as males it is only in an impersonal way and analogous to what we do with many other animals as well as inanimate objects.

On rare occasions Jim could be induced to swell himself up very much and apparently with the greatest effort give utterance to something which sounded like a muffled "Hello." The presence of ladies seemed to furnish a stimulus better calculated to call forth this one expression—Jim's whole vocabulary. I could more frequently induce him to imitate me in saying this word by having a white towel in my hand. It seemed to me that the ladies wearing white could best get Jim to show off his one accomplishment.

As remarked above Jim was most cautious and wary. Any new object placed in his cage he was most slow to make use of. How then was I to determine how rapidly he could learn to open my food-box. I began with him somewhat gradually. On the twelfth of June I made a box like that shown in Fig. 21. To begin with I left off the door as well as the wire from the left end and front of the box. Would Jim eat from the pan placed just inside the door? Not a bit of it. When his food supply was abundant he always hid all that he could not eat. It was hardly ever possible, therefore, to know exactly when you had cut off his supply. Yet in this case I had to coax him nearer and nearer the box by placing the pan well in front and by degrees nearer. Finally I starved him into approaching near enough to stand on tip-toe, stretch his neck, and very suddenly take a bit of food from the pan inside. As an extra inducement I often placed in the pan half a hen's egg of which he seemed to be very fond. With each addition of wire, door, or strings he had to be given time to overcome a new lot of caution.

On July twenty-fourth, about six weeks after I first placed the box inside the cage, my first test was made. The box with door closed was placed in at 2.31 P. M. After forty trials, which

he began to make after the box had been in about two hours, he opened the box by pulling the second string with his beak. Most of his trials were pecks on the wire eyelets by which the strings were attached to the post at the right corner. It was most amusing to see him peck at these and the wire and then in a savage manner pull up great bunches of grass. After the door was opened seven minutes were required for him to take a bit of food from the pan just inside the door.

On the 2d test seven minutes were required and twenty-nine efforts. The 3rd and 4th tests were failures for the reason, largely, that he was not hungry enough. The cause of this was that he had stolen a rat from the caretaker while the latter was feeding the Sparrow-hawk.

From the accompanying table (XII) it may be seen that this Crow like the other birds learned rapidly once he began. He was given more trials per day than most of the others. His many efforts were perhaps due to the fact that his fright caused him to attempt to strike when he was too far off to do so accurately.

On the 20th test he returned to the perch at once on opening the door. Almost immediately he returned to pull the first string some nine times and this with the door already open. On the preceding day he did this once as I started to remove the box from the cage. On the following day, July 30th, he did it four times during one trial. On July 31st,

TABLE XII

Results of experiments on three Crows with a Food-box similar to that used in all the Previous Series.

Trial	Date	Time	Bird opening door	Efforts of Bird opening door and other behavior
1	7-24	34:	Old Crow	40 made first effort after 2 hours. By pulling string B with beak.
2	"-25	7:	"	29
3	"-"	56:	"	94 Failed.
4	"-26	34:	"	3 "
5	"-"	:15	"	3
6	"-27	15:30	"	50
7	"-"	:30	"	6 Most on box and wire.
8	"-"	:30	"	25 Struck at wire eyelets.
9	"-"	1:	"	23 Too frightened to pull hard enough.
18	"-29	:15	"	9 Pulled string with door already open.
19	"-"	:10	"	5
20	"-"	:13	"	4
21	"-"	:13	"	5 Efforts now confined to string E.
22	"-30	:17	"	4 Returned twice and pulled string five times with door open.

TABLE XII—*Continued*

Trial	Date	Time	Bird opening door	Efforts of Bird opening door and other behavior.
23	7-30	:05	Old Crow	2
30	"-31	:40	"	4 Pulled string twice with door open.
33	"-"	:05	"	2 Pulled string twice with door open.
39	8- 1	:05	"	1 Pulled so viciously that string came off.
51	"- 3	:08	"	1 Again pulled string with door open.
52	"- 4	:12	"	2 Young Crow 1 in with old Crow from this on.
53	"-"	:09	"	1
54	"-"	:10	"	1 Young Crow cautious though reared by hand.
60	"- 5	:10	"	1 Food in left front corner. Change was noted but no effect on behavior.
61	"-"	:06	"	1 " " " "
62	"-"	:04	"	1 Food now in left rear corner. No effect.
65	"- 6	:07	"	1 Pulled string with door open six times. Young Crow made first efforts.
66	"-"	:05	"	1 Pulled string with door open.
69	"-"	17:35	Failed	7 String now to left of door.
70	"- 7	10:	"	15 Young Crow 1 to top of box several times.
71	"-"	10:	"	3 Young Crow played a great deal with all parts of box.
72	"-"	4:25	Young Crow 1	1 By accident. Old Crow 3 efforts.
73	"-"	5:45	Old Crow	1 effort to left of door. 38 efforts on old side.
74	"- 8	11:20	"	1 to left of door. 17 on old side.
75	"-"	1.32	"	3 All efforts correctly placed to left of door.
76	"-"	4:15	"	16 " " " "
81	"- 9	1:17	"	2 correctly placed. 11 on old side.
84	"-10	6:03	"	1 Camera in position disturbed him.
85	"-10	7:05	"	1 A painted turtle that had opened the box twice tried again.
110	"-23	2:20	Young Crow 1	3 Pulled top string from top of box. A five day interval between tests.
111	"-"	1:	Old Crow	1
112	"-"	1:50	Young Crow 1	1 Many pulls on the rubber band.
113	"-24	:05	Old Crow	1

TABLE XII—*Continued*

Trial	Date	Time	Bird opening door	Efforts of Bird opening door and other behavior.
114	8-24	:20	Old Crow	2 Strings to left end just round the corner. No efforts by Old Crow in old place.
118	"-25	:40	Young Crow 1	
119	"-"	:50	"	1 Old Crow struck and missed when young Crow pulled same string. Young Crow closes door often by swinging on it.
120	"-26	:15	Old Crow	
121	"-"	:55	Young Crow 1	1 Many efforts all over box. He never ate out of pan. Always waited until I fed him.
122	"-"	:10	Both	Both struck at same time.
123	"-"	:06	Old Crow	1
124	"-"	1:25	Young Crow 1	6
125	"-27	:07	Old Crow	1
126	"-28	:05	"	1
127	"-"	:46	Young Crow 1	
128	"-"	:05	Old Crow	1
129	"-29	:05	Young Crow 1	
130	"-"	:52	"	Old Crow one effort when young Crow 1 came and opened.
131	"-"	:15	Old Crow	1
132	"-30	:30	Young Crow 1	1
133	"-"	1:55	"	3 efforts by Old when Young opened.
134	"-"	:10	"	From ground as Old Crow.
135	"-31	:08	Old Crow	1
136	"-"	2:04	"	2 Strings to rear corner of left end.
137	9- 1	:45	Young Crow 1	1 He searched first for strings in old place.
138	"-"	2:	"	25 Old Crow struck once in 3rd place.
139	"- 2	1:18	"	12 Old Crow 3 in second place.
140	"-"	1:30	Young Crow 2	Many efforts. Continued to pull after door opened.
141	"- 3	7:15	Young Crow 1	Old Crow made 62 efforts. most on door. Very like Young Crow 1
142	"-"	:45	" 2	3
143	"- 4	6:20	Failed	Old Crow struck 17 times at door and rubber band.
144	"-"	:20	Young Crow 2	
145	"- 5	5:30	Old Crow	2 Raining.
146	"-"	1:	"	2 Efforts at door.
147	"-"	:45	"	
148	"- 6	:12	Young Crow 1	
154	"-"	:10	Old Crow	1
155	"- 7	:06	Young Crow 1	
158	"-"	:15	Old Crow	1

TABLE XII—*Continued*

Trial	Date	Time	Bird opening door	Efforts of bird opening door and other behavior.
159	9- 8	:10	Old Crow	1 Young Crow 1 pulled string at once after Old Crow had opened.
160	"-"	1:16	Young Crow 1	
161	"-"	:13	Old Crow	2
163	"-"	: 1	Young Crow 1	11
165	"- 9	:06	Old Crow	1
168	"-"	:30	Young Crow 1	
169	"-"	3:25	"	1 Frightened by wire and posts which replaced boards on rear side.
170	"-"	:50	"	
171	"-"	:25	"	
172	"-"	:15	Old Crow	1 Strings to rear side right corner for next test.
173	"-"	:50	Young Crow 1	6 By pushing bill through front side. Old Crow strongly inclined to work on left side.
174	"-"	1:30	Old Crow	33 He struck as Young Crow in previous trials and opened in same manner.
175	"-"	1:40	"	2 hard blows on door.
176	"-"	3:30	Young Crow 1	Lifted door up with claws. Old Crow made 15 efforts in front.
177	"-10	3:	"	Lifted door up with claws. Old Crow 30 efforts in front.
178	"-"	3:50	"	1 correctly placed. Many otherwise.
179	"-"	1:30	"	By striking through wire at end of latch. Old Crow 15 efforts in front.
180	"-"	1:05	"	Correctly placed.
181	"-"	2:	"	Many efforts in front.
199	"-15	1:30	"	1 Young Crow is fed and old allowed to get hungry. Old Crow made one attempt but failed.
200	"-"	:50	Old Crow	1 From preceding test to close, the Old Crow is often induced to open door by feeding Young Crow 1 preceding each test.
201	"-"	:30	Young Crow 1	1 with foot.
203	"-17	4:	Old Crow	Many trials necessary.
205	"-18	: 5	Young Crow 1	1
206	"-"	1:50	Old Crow	4 Came round from rear often to see if door had been opened by efforts just made.
207	"-19	2:30	Young Crow 1	With foot.
208	"-"	:10	Old Crow	
214	"-21	:10	"	

again once each in two different tests, twice again on Aug. 3rd, and again on Aug. 6th. On Aug. 6th the pan of food was removed from just behind the strings to the front left corner. He noted the change but it did not affect his behavior. After three tests the food was placed in the left rear corner but this again made no change in his behavior. It is understood, of course, that each time after the door was opened I placed the food out next the door-sill so that Jim could get it. He would probably starve before entering the box.

On August 5th one of two young crows that were taken from the nest about the first of June and reared by hand was turned into the cage with Jim. The accompanying figure 20, gives a good idea of the tameness of these crows, and yet they were, when a month or so old, somewhat more fearful than as shown in the photograph. If a stranger fed them they might refuse to eat from his hand until very hungry. This young one, which we shall designate as Young Crow 1, was at first inclined to be rather cautious in his approach to the box. On the second day, however, he struck at the wire and nail heads.

On this same day the strings were removed from their place at the right of the door, place 1, and placed to the left of the door, place 2. This change was identical with that made in the box for the birds in Series A and others. It will be seen from the table that nothing was done during seventeen and one-half minutes except some efforts on the wire eyelets where the old strings had been attached. In the 2nd test Jim made fifteen unsuccessful trials in ten minutes and then returned to his perch. In the following trial of ten minutes Young Crow 1 played a good deal on top of the box. The young crows had shown signs of play when in their own cage. Jim made three unsuccessful efforts, but failed in ten minutes. Young Crow 1 opened the door by pulling the top string when standing on top of the box. Jim had already made five attempts during this trial. In the succeeding trial Jim worked very hard. He was successful only after making forty-eight vain efforts on the old side. He seemed to have very strong associations with the old place and not for the strings. The box is so made that the door swings back against these strings. Jim's great fear may well have been increased by this fact. The time required for the next trial was longer. Seventeen errors, all made by working on the old place, were recorded against Jim in this trial. Young Crow 1 was working on the top of the door. He struck it with the beak and jumped back suddenly each time.

By this time Jim showed signs of learning to work on the strings in the new position. There was some little interruption during a few tests at about this time. This was caused by the



FIG. 20

The two Young Crows as they were usually fed. Such tameness was to be had as they grew older only with the most constant feeding and by the one person who cared for them.

fact that a turtle had been eating from the pan which contained the bird food, and in fact opened the door a few times by crawling up between the strings and the wire.

The Young Crow 1 worked at the box a great deal. He seemed to enjoy pulling and tugging at the rubber band which

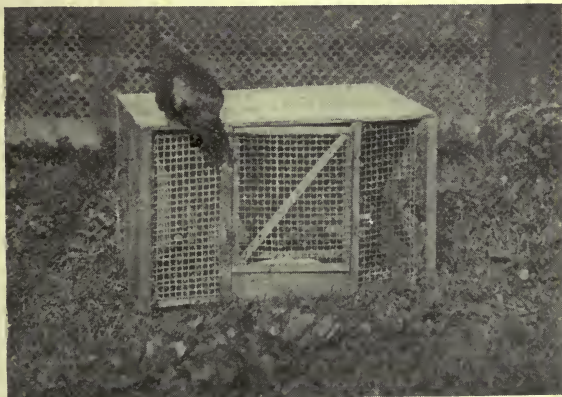


FIG. 21

The strings were now in Place 4 on left end. Young Crow 1 is shown here as he happened to find a way to open the door which I had not foreseen. He pushed his beak through the wire and pulled the strings as they run from the latch to Place 4. See the text (p. 63) for the effect of this on the Old Crow.

was used to pull the door open when the latch was raised. He had opened the door some three times in all prior to the 123rd test. At this time the strings were removed to the niche in the left end, place 3, but just around the corner from where they were. As may be seen from the results, Jim made this transition with very little if any difficulty. He continued to do so for four more tests when Young Crow 1 opened the door from the top of the box. In the next Jim struck at the string but missed, when Young Crow 1 at once pulled the same string. Both worked at exactly the same time in the third trial following. In succeeding trials the Young Crow often opened only after Jim had made one attempt.

On the 146th trial the strings were changed to the rear position on the left end. Jim opened the first time, but had to make three different approaches. He looked for the strings in the old position, as did the Young Crow 1 in the next trial, before working them in the new location. In the next two trials Jim made attempts on the old positions.

From the standpoint of imitation the results of this series now come to be more interesting. Young Crow 1 pecked at the top of the door a great deal. Young Crow 2 had now been placed in the cage. Jim struck the door sixty-two times or until Young Crow 1 pulled the string and opened it. The extent to which the latter understood what he was doing may be guessed from the fact that he went on pulling after the door was open. Young Crow 2 was successful next time, and he, too, continued to pull as the other had done. In the next Jim made seventeen errors by working on the door and the rubber band.

Several trials later Young Crow 1 stood on the ground and opened the door as Jim did. It is very difficult to classify this as imitation, though he did it a number of times, for the reason that this crow was so versatile. He did everything to the box in every way in which he could peck or pull at any part of it. He even went so far as to stand on the ground and pull the same string as Jim. Inasmuch as he had the same day stood on top and struck at the top string immediately after Jim opened by using the lower string we may call it imitation according to the more difficult criterion. It involved a change from previous behavior.

In order to make a place for the strings on the rear side of the box so that this series should correspond exactly with that on the Sparrows, etc., Series A, the boards were removed and posts and wire were put in on the rear, Fig. 22. This change made Jim and even Young Crow 1 afraid whenever they caught sight of the wire. This caused the times to be somewhat longer.

For the 4th trial following this change the strings were placed on the rear at the right corner. There now occurred a fortunate conjunction of circumstances more than once which has given me my best results on the subject of imitation in birds. Instead of opening the door as I meant for him to do, Young Crow 1, after about a dozen efforts, put the beak through the wire just to the right of the door in front and pulled one of the strings running vertically from the latch to the under surface of the top of the box, Fig. 21. The door opened and following his playful habit he jumped to the top



FIG. 22

Young Crow 1 at his favorite play with the box. He was swinging on the door. When he jumped off the door usually closed and he or the Old Crow must open again. This is important for it brings the tests in rapid succession, a very favorable condition for imitation:

of the door and swung back and forth a few times, Fig. 23. In hopping off the door, as he always did in a few seconds, he closed it and thus made possible another test *at once* after Jim had seen him open the door from in front. In spite of the fact that Jim had shown strong inclination to go to the left end, he now made most of his thirty efforts on the front and really succeeded in opening the door in the same way as the young one had done. In the next trial Jim opened by two hard blows on the door itself, for I had placed the strings so that they could not be reached through the wire.

For the following trial I had changed the box so that the door did not open so easily by striking against it. However, Jim struck it fifteen times and the Young Crow 1 outwitted me again by pure accident in lifting the door up when his claws extended over the edge of the box and caught in the upper

edge of the door. In the following trial Young Crow 1 opened in much the same indirect manner with the claws, but only after Jim had made thirty efforts with his bill. Again there were many efforts on the door, but this trial the young one pulled the strings as he should. This was followed by a test in which Jim made about fifteen unsuccessful efforts with his beak, but Young Crow 1 succeeded by striking through the wire at the left end of the latch. Previous to this I had changed the box so that no amount of pecking could open it, but now I had to cut off the end of the latch so that it did not extend out from the post to which the door was hung. From the fact that in the next trial this Young Crow 1 made many attempts on the door and front of the box, we infer that his records would give us a good curve of learning did he not alternate so much with Jim and were there not so many conditions necessary for Jim and tests for imitation, which were hardly calculated to permit of good results for the Young Crow 1 as a subject of mere learning tests.

From this point on to the close of these tests this Young Crow 1 opened the box except when he was fed just preceding each test and Jim was allowed to go hungry. Even then the former would sometimes be the first to perform the necessary act. See Fig. 23. This would indicate that his opening the box was often a result of what we might term his playful activity.

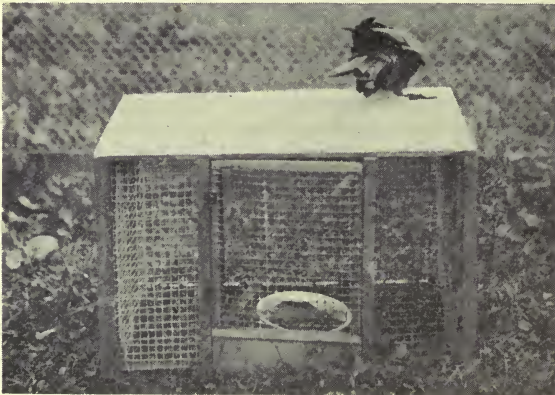


FIG. 23

Young Crow 1 shown opening the door by pulling String C in Place 5. The Old Crow was induced later to work in this place, but did so in his own way. The Young Crow was fed from the hand just before each test and as a rule made no attempts to open the box.

It is of interest to note that when Jim made a rapid but un-

successful pass at the string, he would often run around to the door, evidently expecting that his act had brought the desired result.

It would seem that in the present series there was a happy conjunction of just those factors needed to call forth imitation. May we not say that these are just the conditions and factors which render the experiment less artificial and less far removed from those natural conditions in which so many have taken it for granted that intelligent imitation did find a large place? The present writer is persuaded that what he needs with other birds and animals is a more or less exact duplication of the conditions which obtained with these crows.

Jim followed the example given by young Crow 1. He did not simply perform an act in the same way as his model did but in that part of the box worked upon as well as the definite things done he was seen to go against an old habit, against that which he was strongly inclined to do. He did so probably because previous methods of his own had brought the desired result.

Without great caution on the part of this older bird, frequent changes in the fastenings on the box, without the incessant playful efforts on the part of the young crow, indeed, without the imperfections in the box which made it possible to open it in an unexpected way, it is difficult to see how such a clear case of imitation could have been obtained. That this is the best series which the present writer has to submit is largely due to results unforeseen but which nevertheless may serve us all the better as proof of that which we were in search of. But it may be said that many of the changes which the Old Crow made in his behavior were simple reversions to a previous method of his own. True this may be in many cases and yet this very point is involved if we analyze the conditions necessary for the appearance of imitation. Where could the idea or impulse of the movement to be made in imitation come from if the animal or child had had no previous experience of its own to use at least as a basis. In an important way this previous experience is just what has not been given to animals by earlier students of imitation. The lack of this previous experience in sufficient amount is perhaps the chief reason for the fact that it is some months—four to six—before the higher stages of imitation appear in the human child.

VI. DISCUSSION OF RESULTS

The origin and development of social life in the animal as well as in the human world have long been considered problems by scientists, whether biologists, sociologists, psychologists, or educators. Imitation of some kind or other, all

would agree, constitutes one and only one of the chief factors in this problem. The interpretation of our findings, stated in the previous sections, may be dealt with briefly here.

Let us consider the different kinds of imitation. Principal Morgan, that peerless thinker in the problems of Animal Behavior, has proposed the following:

(1) Mimicry, which lies below the level of imitation and which (the present writer would add) is determined by forces wholly outside the individual. Mimicry is determined for the individual in the previous history, or, let us say, experience of the species, either through environmental or organic conditions. Some recent investigations give promise of serious revision of our thought with reference to just why the walking-stick, for example, is so like the branch on which it is found. At any rate, it is the experience working through natural selections and other ways, and not at all necessarily through transmission of acquired characters, that pre-determines Mimicry in the individual. The reason for the writer's use of the word experience will appear later.

(2) Instinctive Imitation. This is again inherited though now the emphasis, because of the meaning of the word "Instinctive," is on function rather than structure. Partly because it is function rather than structure, but also because of the nature of the external stimulus which sets the instinct going, it is to be expected that what the individual imitates instinctively will not be copied in so fixed a manner as mimicry demands. There will be a good deal of the individual in it. Nevertheless such instinctive imitation is done readily and automatically, and is determined by the experience of the species. It belongs in the same class with mimicry, but offers more of plasticity of action to the individual. They therefore have the same relation as Hobhouse has pointed out for Reflex Action and Instinct. Examples of instinctive imitation are to be found in the "following instinct," which is very well illustrated by the observations described on page 6. Many, if not all, of the instances of imitations of song in birds are examples of instinctive imitation. The calls and songs of birds are very intimately associated with the mating as well as other instincts. Some of these might well become intelligent after being performed the first few times.

(3) Intelligent Imitation. Until the appearance of his most recent work on Animal Behavior this formed the last and highest part of Morgan's classification. In this book, however, this author makes a distinction which the present writer deems most necessary and useful, but which needs to be carried further.

All other earlier writers separate too widely instinctive and

intelligent imitation. Most have made intelligent imitation synonymous with "reflective," "intentional," "voluntary," "persistent" and hence this wide separation was very natural and even necessary. Inasmuch as students of animal psychology have agreed to use intelligence to mean "profiting by experience," and not reason, it would seem that intelligent imitation should be kept apart from the words which seem to imply in the animal mind something which is akin to, if not identical with, reasoning. The writer then proposes, and this is done chiefly because the facts seem to require it, as a third class that of intelligent imitation.

(4) Reflective, Intentional or Voluntary. This class involves an analysis of which, as can be shown, the bird is not capable. At least the observations which I have made would seriously call it in question. Such are those errors which have occurred in almost every one of the series described—pulling the strings repeatedly with the door already open, the indirect method used by the White-throated Sparrow, the errors of the Bluebird even in the Memory series, the errors and following up of misleading acts by the Orioles and Crows, etc. A fuller discussion of the significance of these facts would be both interesting and profitable in affording us an insight into bird mind, but it is hardly necessary here.

We have seen that mimicry and instinctive imitation are predetermined for the individual by the experience of the species. Has it not been rather futile for us to expect that one animal should imitate another in the doing of some act which each individual animal must learn by the "trial and success" method and this only? Most students of animals agree that it is by individual experience that such a thing is first learned. Then here is a test such as pulling a string to open a door to get food, which is extremely artificial from the animal point of view. How artificial it is only those can appreciate who have observed the enormous number of different things birds will do before pulling such a string much less to imitate another's pulling it.

The criterion which is proposed, stated on page 8, makes the test more difficult for the animal. He must change his own method for that of another. All my work with birds would lead me to expect that each species will probably open the door, such as I have used most, in a few limited ways. One feels that the Cowbird will use the beak or stand on the floor and pull with one claw. Several English Sparrows who have been very wild have used the same method of flying out from the top of the box and barely alighting on the strings. Yet even this fear may be an incentive to their following a copy set by another bird, and changing from their former method to the

one to be imitated. If other higher animals such as House Mice were worked with, I should deem it very necessary to make it certain that odor could in no way be used as a constantly leading stimulus for the animal doing the imitating. With birds such precaution is hardly necessary. It is considerations such as these which seem to demand a change of method in order that we obtain attention and interest from the animals, make our experiments more natural, and use a criterion which is more rigid so that what we have left, after we have rejected all that does not measure up, shall be really true.

As is evident from the detailed results, we do get examples of intelligent imitation. I do not call it voluntary, reflective, rational, or intentional. If animals are intelligent but not rational, this is all that we should expect. Such imitation is as stupid, as blindly dictated by accident and satisfaction and discomfort, and as impulsive as the intelligent act *per se*.

The writer hopes at some later time to show that much of human learning is analogous, not necessarily homologous, with animal learning. Both learn by "trial and success," the happy accident and the painful result method. Indeed man does not reason by far as much as our popular notions concerning this question would lead us to suppose. If this is true, then imitation in children and the higher animals ought to bear some instructive analogies.

The writer has three children. One is now two years old, another nine years and another eleven. Child psychologists record the fact that imitation appears first in a noticeable way about the fifth month. They mean, of course, the appearance of reflective, or better perhaps, intelligent imitation. I have several observations on the youngest child for the fifth month and thereafter which show clearly as it seems to me, that her imitations took the form of those things, or closely similar to experiences, which I had noted she had been having in an entirely spontaneous way. She imitated the nine-year-old in producing a high tone long drawn out. But it was not until after several repetitions that she knew she was doing so. This knowledge was indicated by the quite marked change in facial expression. So with a half dozen other cases in which there were pretty clear signs of imitation. All were based on previous experience, however. It has been observed often, as no doubt has been done by many others, that the baby shows much more interest and attention to what is done by the sister nine years of age than to the behavior of adults.

After the child referred to above had performed the imitation a few times, each performance being followed by all kinds of approval by one or more adults, and renewed efforts to elicit more, there were signs of a consciousness on the child's part

which promised to cause such acts to become rapidly imitation of a reflective, voluntary or intentional kind. As they first occurred they were to me examples of intelligent imitations, prepared for by the child's own experience.

It may not be amiss to ask what it is which characterizes those acts which adult persons imitate in an unreflective way? Does the preparation we have had, the previous experience, have the same effect as we think it has for these birds, only for the latter in a far more restricted manner?

There are, then, many reasons for believing that it is possible by giving a bird previous experience with the object to be worked with to get that sort of imitation which may be called intelligent. This probably is on a little higher level than instinctive, and yet does not mean the same as reflective, intentional, or voluntary. The writer hastens to say, however, that he really believes that any classification of the above kind does not adequately represent the facts. The observed facts in the animal are of all stages of instinctive and intelligent and in the child these two are not left behind when they emerge into the reflective. In this instinctive basis we find a satisfactory explanation of the facts concerning imitation in bird songs. There is probably no distinction to be made here between that which is instinctive from that which is intelligent. Were it possible to come to the bird with an act for it to imitate, which would appeal as naturally, spontaneously and repeatedly as these call notes and songs do, then we should probably get intelligent imitation as unmistakably and easily. This is difficult, but the writer feels that with the long series of tests under the conditions imposed and the demands of the more difficult criterion, we may be more certain of getting examples of intelligent imitation; but hardly that which is distinctively human,—the reflective, intentional or voluntary.

VIII. SUMMARY

1. Several long additional series of experiments on the intelligence of the English Sparrow and Cowbird have been obtained. The rate of learning is slower than that found earlier, but my present conditions were different inasmuch as the primary object here was to get results on imitation. This should be kept in mind by the reader in connection with all the tests here described. The Cowbirds spent much of their time in looking for parasites on the Sparrows and parts of the cage.

2. The male English Sparrow showed signs of imitating the female Sparrow, and more unmistakable signs of imitating the Cowbird. His latest change in behavior satisfied the requirements of the criterion which the writer from previous

results has been led to adopt. For a statement of this criterion see page 8. We not only obtain a more rigid standard, but we are more certain that Bird No. 1 really sees, is interested in, and attends to what the other is doing. It has had such experience with the apparatus, that it is more likely to be interested in what is done in order to open the box.

3. If the Cowbirds did start a series, as they often did, the English Sparrows after a few tests displaced them.

4. This may be accounted for by the fact that the English Sparrow is far more active and though much smaller, often drove the Cowbirds away and held them at bay even after a bitter struggle.

5. The Junco learned to open the food-box, but not until after the female English Sparrow had taken the lead. There was keen rivalry and fighting between them. There was probable imitation of each by the other of the intelligent sort.

6. The White-throated Sparrow and Field Sparrow learned to open the food-box though not so rapidly as some of the other birds. This slowness may be due to the indirect method of the former, to the small size of the latter, and to the fact that both were tried in a cage near the entrance to all the cages, and hence were disturbed a good deal.

7. A young Song Sparrow for a few tests showed signs of rather rapid learning. There were a few uncertain signs of imitation of the intelligent sort with the White-throated, Field, and Song Sparrows and the Junco.

8. In a long series of tests on a pair of Bluebirds, White-crowned Sparrows, and two each of Juncos and Tree, Fox, and Song Sparrows, the best fighters were the ones to open the door early in the series. The male Bluebird early began to lead and finally was the only one to do it. Here again there were some uncertain imitative acts on the part of each Bluebird, but chiefly by the female. A memory series for these Bluebirds showed that they had forgotten little in one month. It also revealed some errors on their part which indicated that such memory of the place to alight upon did not mean that they had at any time analyzed the mechanism of the box. Some learning tests on another male Bluebird, who was alone in the cage, showed that he was very slow in starting.

9. A long series of tests with a pair of Blue Jays gave no results on imitation, but did give good evidence of the male's power to learn. But when the strings were changed he did not adapt himself as readily as the English Sparrow. However, it was still possible for the Jay to use his old method. This of course makes it impossible to generalize from these tests.

10. In experiments on three Cowbirds,—two males and one female,—two young English Sparrows and a male Balti-

more Oriole, one of the male Cowbirds first opened the door but the Oriole soon displaced him and learned to do it in his own way. Later a more pugnacious Oriole was placed in the cage. He soon was the one who opened the door. There were signs of imitation here, although he did not use the method Oriole No. 1 used. Later Oriole No. 1 imitated Oriole No. 2.

11. When the strings were changed these birds did not show as ready adaptation as the English Sparrow. This may be due to the fact that it was still possible for Oriole No. 1 to use his old method which he did.

12. The old crow was so cautious that beginning tests with him were long, and there were many efforts. He learned rapidly when once started. He adapted himself to the first change in the position of the strings on the box with less readiness than the female English Sparrow. With the second and third change his results were quite comparable with the female English Sparrow's. For the fourth change the Young Crow 1 interfered. The crows, largely because the Young Crow 1 in his incessant play with the box aided materially in bringing about favorable conditions, furnish us with the best examples of imitation. This is so from the fact that the old crow was led through imitation to change his method of opening the door. The more rigid and difficult criterion is thus shown to be practicable here as well as with the Orioles, the Junco and English Sparrow and Cowbird.

13. In those series where it was possible to obtain data it is somewhat evident that birds of the same species imitated each other more readily than the members of a different species. Yet the latter has occurred twice at least.

14. In the case of nearly every bird which really learned to open the door, we have found repeated pulling of the strings with the door already open. This with the behavior of the birds when the strings were changed to a new position and opening the door in the old place, if such was possible, point to the fact that there was not much analysis of the relations of the situation or parts of the box.

15. In view of what has just been said, it would seem advisable to suggest Intelligent Imitation as a new class leaving the words "reflective," "voluntary," or "intentional," for that which is probably to be classed as distinctively human.

Some of the results of the present paper were presented before the Section on Animal Behavior at the meeting of the International Congress of Zoölogy which met in Boston, 1907. All of the results of this paper together with others were made the basis of a report at the Chicago meeting of the American Psychological Association (1907-08). In a somewhat modified form this same study was awarded the Walker Prize of the Boston Society of Natural History in 1908.

THE ŒDIPUS-COMPLEX AS AN EXPLANATION OF HAMLET'S MYSTERY: A STUDY IN MOTIVE

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English-speaking psychologists have as yet paid relatively little attention to the study of genius and of artistic creativeness, at least so far as the method of analysing in detail the life-history of individual men of genius is concerned. In Germany, stimulated by Moebius' example, many workers have obtained valuable results by following this biographical line of investigation. Within the past few years this study has been infused with fresh interest by the luminous writings of Professor Freud, who has laid bare some of the fundamental mechanisms by which artistic and poetic creativeness proceeds.¹ He has shewn that the main characteristics of these mechanisms are common to many apparently dissimilar mental processes, such as dreams, wit, psycho-neurotic symptoms, etc.² and further that all these processes bear an intimate relation to fantasy, to the realisation of non-conscious wishes, to psychological "repression" (*Verdrängung*), to the re-awakening of childhood memories, and to the psycho-sexual life of the subject. His analysis of Jensen's novel *Gradiva* will serve as a model to all future studies of the kind.

It is generally recognised that although great writers and poets have frequently made the most penetrating generalisations in practical psychology, the world has always been slow to profit by their discoveries. Of the various reasons for this fact one may here be mentioned, for it is cognate to the present argument. It is that the artist is often not distinctly aware of the real meaning of what he is seeking to express, and is never aware of its source. The difficulty experienced by the artist in arriving at the precise meaning of the creation to which he is labouring to give birth has been brilliantly demonstrated by Bernard Shaw³ in the case of Ibsen and Wagner. The artist

¹ Freud: *Der Wahn und die Träume in W. Jensen's Gradiva*, 1907. *Der Dichter und das Phantasieren*. *Neue Revue*, 1908. No. 10, S. 716.

² Freud: *Traumdeutung*, 1900. *Der Witz und seine Beziehung zum Unbewussten*, 1905. *Drei Abhandlungen zur Sexualtheorie*, 1905. *Sammlung kleiner Schriften zur Neurosenlehre*, 1906. *Zweite Folge*, etc.

³ Bernard Shaw: *The Quintessence of Ibsenism*, 1891. *The Perfect Wagnerite*. 2nd ed., 1901.

works under the impulsion of an apparently external force; indeed, being unaware of the origin of his inspiration, it frequently happens that he ascribes it to an actual external agency, divine or otherwise. We now know that this origin is to be found in mental processes which have been forgotten by the subject, but which are still operative; in Freud's language, the creative output is a sublimated manifestation of various thwarted and "repressed" wishes, of which the subject is no longer conscious. The artist, therefore, gives expression to the creative impulse in a form which satisfies his internal need, but in terms which he cannot translate into easily comprehensible language; he must express it directly as it feels to him, and without taking into consideration his possible audience. An evident corollary of this is that the farther away the artist's meaning from the minds of those not in possession of any of his inspiration the more difficult and open to doubt is the interpretation of it; hence the flood of quite silly criticism that follows in the wake of such men as Schopenhauer and Nietzsche.

It is to be expected that the knowledge so laboriously gained by the psycho-analytic method of investigation would prove of great value in the attempt to solve the psychological problems concerned with the obscurer motives of human action and desire. In fact one can see no other scientific mode of approach to such problems than through the patient unravelling of the deeper and hidden layers of the mind by means of the dissecting procedures employed in this method. The stimulating results already obtained by Muthmann,¹ Rank,² Riklin,³ Sadger,⁴ Abraham⁵ and others are only a foretoken of the applications that will be possible when this method has been employed over a larger field than has hitherto been the case.

The particular problem of Hamlet, with which this paper is concerned, is intimately related to some of the most frequently recurring problems that are presented in the course of psycho-analysis, and it has thus seemed possible to secure a new point of view from which an answer might be offered to questions that have baffled attempts made along less technical routes. Some of the most competent literary authorities have freely acknowledged the inadequacy of all the solutions of the prob-

¹Muthmann: Psychiatrisch-Theologische Grenzfragen. Zeitschr. f. Religions-psychologie. Bd. I. Ht. 2 u. 3.

²Otto Rank: Der Künstler. Ansätze zu einer Sexual-psychologie, 1907. Der Mythos von der Geburt des Helden, 1909.

³Riklin: Wunscherfüllung und Symbolik im Märchen, 1908.

⁴Sadger: Konrad Ferdinand Meyer. Eine pathographisch-psychologische Studie, 1908. Aus dem Liebesleben Nicolaus Lenaus, 1909.

⁵Abraham: Traum und Mythos. Eine Studie zur Völkerpsychologie, 1909.

lem that have up to the present been offered, and from a psychological point of view this inadequacy is still more evident. The aim of the present paper is to expound an hypothesis which Freud some nine years ago suggested in one of the footnotes to his *Traumdeutung*;¹ so far as I am aware it has not been critically discussed since its publication. Before attempting this it will be necessary to make a few general remarks about the nature of the problem and the previous solutions that have been offered.

The problem presented by the tragedy of Hamlet is one of peculiar interest in at least two respects. In the first place the play is almost universally considered to be the chief masterpiece of one of the greatest minds the world has known. It probably expresses the core of Shakspeare's philosophy and outlook on life as no other work of his does, and so far excels all his other writings that many competent critics would place it on an entirely separate level from them. It may be expected, therefore, that anything which will give us the key to the inner meaning of the play will necessarily give us the clue to much of the deeper workings of Shakspeare's mind. In the second place the intrinsic interest of the play is exceedingly great. The central mystery in it, namely the cause of Hamlet's hesitancy in seeking to obtain revenge for the murder of his father, has well been called the Sphinx of modern Literature.² It has given rise to a regiment of hypotheses, and to a large library of critical and controversial literature; this is mainly German and for the most part has grown up in the past fifty years. No review of the literature will here be attempted, for this is obtainable in the writings of Loening,³ Döring,⁴ and others, but the main points of view that have been adopted must be briefly mentioned.

Of the solutions that have been offered many will probably live on account of their very extravagance.⁵ Allied if not belonging to this group are the hypotheses that see in Hamlet allegorical tendencies of various kinds. Thus Gerth⁶ sees in

¹ S. 183.

² It is but fitting that Freud should have solved the riddle of this Sphinx, as he has that of the Theban one.

³ Loening: *Die Hamlet-Tragödie Shakespeares*, 1893. This book is warmly to be recommended, for it is by far the most critical work on the subject.

⁴ Döring: *Ein Jahrhundert deutscher Hamlet-Kritik. Die Kritik*, 1897, Nr. 131.

⁵ Such, for instance, is the view developed by Vining (*The Mystery of Hamlet*, 1881) that Hamlet's weakness is to be explained by the fact that he was a woman wrongly brought up as a man.

⁶ Gerth: *Der Hamlet von Shakespeare*, 1861.

the play an elaborate defence of Protestantism, Rio¹ and Spanier² on the contrary a defence of Roman Catholicism. Stedefeld³ regards it as a protest against the scepticism of Montaigne, Feis⁴ as one against his mysticism and bigotry. A writer under the name of Mercade⁵ maintains that the play is an allegorical philosophy of history; Hamlet is the spirit of truth-seeking which realises itself historically as progress, Claudius is the type of evil and error, Ophelia is the Church, Polonius its Absolutism and Tradition, the Ghost is the ideal voice of Christianity, Fortinbras is Liberty, and so on. Many writers, including Plumptre⁶ and Silberschlag,⁷ have read the play as a satire on Mary, Queen of Scots, and her marriage with Bothwell after the murder of Darnley, while Elze,⁸ Isaac,⁹ and others have found in it a relation to the Earl of Essex's domestic history. Such hypotheses overlook the great characteristic of all Shakspeare's works, namely the absence in them of any conscious tendencies, allegorical or otherwise. In his capacity to describe human conduct directly as he observed it, and without any reference to the past or future evolution of motive, lay at the same time his strength and his weakness. In a more conscious age than his or ours Shakspeare's works would necessarily lose much of their interest.

The most important hypotheses that have been put forward are sub-varieties of three main points of view. The first of these sees the difficulty in the performance of the task in Hamlet's temperament, which is not suited to effective action of any kind; the second sees it in the nature of the task, which is such as to be almost impossible of performance by any one; and the third in some special feature in the nature of the task which renders it peculiarly difficult or repugnant to Hamlet.

The *first* of these views, which would trace the inhibition

¹ Rio: Shakespeare, 1864.

² Spanier: Der "Papist" Shakespeare im Hamlet, 1890.

³ Stedefeld: Hamlet, ein Tendenzdrama Shakespeares gegen die skeptische und kosmopolitische Weltanschauung des M. de Montaigne, 1871.

⁴ Feis: Shakspeare and Montaigne, 1884. The importance of Montaigne's influence on Shakspeare, as shewn in Hamlet, was first remarked by Sterling (London and Westminster Review, 1838, p. 321), and has been clearly pointed out by J. M. Robertson in his book, Montaigne and Shakspeare, 1897.

⁵ Mercade: Hamlet; or Shakespeare's Philosophy of History, 1875.

⁶ Plumptre: Observations on Hamlet, being an attempt to prove that Shakespeare designed his tragedie as an indirect censure on Mary, Queen of Scots, 1796.

⁷ Silberschlag: Shakespeare's Hamlet. Morgenblatt, 1860, Nr. 46, 47.

⁸ Elze: Shakespeare's Jahrbuch, Bd. III.

⁹ Isaac: Shakespeare's Jahrbuch, Bd. XVI.

to some defect in Hamlet's constitution, was independently elaborated more than a century ago by Goethe,¹ Schlegel² and Coleridge.³ Owing mainly to Goethe's advocacy it has been the most widely-held view of Hamlet, though in different hands it has undergone innumerable modifications. Goethe promulgated the view as a young man and when under the influence of Herder,⁴ who later abandoned it.⁵ It essentially maintains that Hamlet, for temperamental reasons, was fundamentally incapable of decisive action of any kind. These temperamental reasons are variously described by different writers, by Coleridge as "overbalance in the contemplative faculty," by Schlegel as "reflective deliberation—often a pretext to cover cowardice and lack of decision," by Vischer⁶ as "melancholic disposition," and so on. A view fairly representative of the pure Goethe school would run as follows: Owing to his highly developed intellectual powers, and his broad and many-sided sympathies, Hamlet could never take a simple view of any question, but always saw a number of different aspects and possible explanations of every problem. A given course of action never seemed to him unequivocal and obvious, so that in practical life his scepticism and reflective powers paralysed his conduct. He thus stands for what may roughly be called the type of an intellect over-developed at the expense of the will, and in Germany he has frequently been held up as a warning example to university professors who shew signs of losing themselves in abstract trains of thought at the expense of contact with reality.⁷

There are at least three grave objections to this view of Hamlet's hesitancy, one based on general psychological considerations and the others on objective evidence furnished by the play. It is true that at first sight increasing scepticism and reflexion apparently tend to weaken motive, in that they tear aside common illusions as to the value of certain lines of

¹ Goethe: *Wilhelm Meister's Lehrjahre*, 1795.

² Schlegel: *Vorlesungen über dramatische Kunst und Litteratur*, III, 1809.

³ Coleridge: *Lectures on Shakespeare*, 1808.

⁴ Herder: *Von deutscher Art und Kunst*, 1773.

⁵ Herder: *Aufsatz über Shakespeare im dritten Stück der Adrastea*, 1801.

⁶ Vischer: *Kritische Gänge*. N. F., Ht. 2, 1861.

⁷ See for instance Köstlin: *Shakespeare und Hamlet*. *Morgenblatt*, 1864, Nr. 25, 26. Already in 1816 Börne in his *Dramaturgischen Blättern* had cleverly developed this idea. He closes one article with the words "Hätte ein Deutscher den Hamlet gemacht, so würde ich mich gar nicht darüber wundern. Ein Deutscher braucht nur eine schöne, leserliche Hand dazu. Er schreibt sich ab und Hamlet ist fertig."

conduct. This is well seen, for instance, in a matter such as social reform, where a man's energy in carrying out minor philanthropic undertakings wanes in proportion to the amount of clear thought he devotes to the subject. But closer consideration will shew that this debilitation is a qualitative rather than a quantitative one. Scepticism leads to a simplification of motive in general, and to a reduction in the number of those motives that are efficacious; it brings about a lack of adherence to certain conventional ones rather than a general failure in the springs of action. Every student of clinical psychology knows that any such general weakening in energy is invariably due to another cause than intellectual scepticism, namely, to the functioning of abnormal unconscious complexes. This train of thought need not here be further developed, for it is really irrelevant to discuss the cause of Hamlet's general aboulia if, as will presently be maintained, this did not exist; the argument, then, must remain unconvincing except to those who already accept it. Attempts to attribute Hamlet's general aboulia to less constitutional causes, such as to grief due to the death of his father and adultery of his mother,¹ are similarly inefficacious, for psycho-pathology has clearly demonstrated that such grief is in itself quite inadequate as an explanation of this condition.

Unequivocal evidence of the inadequacy of the hypothesis under discussion may further be obtained from perusal of the play. In the first place there is every reason to believe that, apart from the task in question, Hamlet is a man capable of very decisive action. This could be not only impulsive, as in the killing of Polonius, but deliberate, as in the arranging for the death of Guildenstern and Rosencrantz. His biting scorn and mockery towards his enemies, and even towards Ophelia, his cutting denunciation of his mother, his lack of remorse after the death of Polonius, are not signs of a gentle, yielding or weak nature. His mind was as rapidly made up about the organisation of the drama to be acted before his uncle, as it was resolutely made up when the unpleasant task had to be performed of breaking with the uncongenial Ophelia. He shews no trace of hesitation when he stabs the listener behind the curtain,² when he makes his violent onslaught on the pirates, leaps into the grave with Laertes or accepts his challenge to the fencing match, or when he follows his father's ghost on to the battlements; nor is there any lack of determination in his resolution to meet the ghost;

¹ A suggestion first proffered by Herder. *Op. cit.*, 1801.

² I find Loening's argument quite conclusive that Hamlet did not have the king in his mind when he committed this deed. (*Op. cit.*, S., 242-244, 362-363.)

"I'll speak to it, though hell itself should gape
And bid me hold my peace,"

or in his cry when Horatio clings to him,

"Unhand me, gentlemen;
By heaven, I'll make a ghost of him that lets me;
I say, away!"

On none of these occasions do we find any sign of that paralysis of doubt which has so frequently been imputed to him. On the contrary, not once is there any sort of failure in moral or physical courage except only in the matter of the revenge. In the second place, as will later be expounded, Hamlet's attitude is never that of a man who feels himself not equal to the task, but rather that of a man who for some reason cannot bring himself to perform his plain duty. The whole picture is not, as Goethe depicted, that of a gentle soul crushed beneath a colossal task, but that of a strong man tortured by some mysterious inhibition.

Already in 1827 a protest was raised by Hermes¹ against Goethe's interpretation, and since then a number of hypotheses have been put forward in which Hamlet's temperamental deficiencies are made to play a very subordinate part. The *second* view here discussed goes in fact to the opposite extreme, and finds in the difficulty of the task itself the sole reason for the non-performance of it. This view was first hinted by Fletcher,² and was independently developed by Klein³ and Werder.⁴ It maintains that the extrinsic difficulties inherent in the task were so stupendous as to have deterred any one, however determined. To do this it is necessary to conceive the task in a different light from that in which it usually is conceived. As a development largely of the Hegelian teachings on the subject of abstract justice, Klein, and to a lesser extent Werder, contended that the essence of Hamlet's revenge consisted not merely in slaying the murderer, but of convicting him of his crime in the eyes of the nation. The argument, then, runs as follows: The nature of Claudius' crime was so frightful and so unnatural as to render it incredible unless supported by a very considerable body of evidence. If Hamlet had simply slain his uncle, and then proclaimed, without a shred of supporting evidence, that he had done it to avenge a fratricide, the nation would infallibly have cried out upon him, not only

¹ Hermes: Ueber Shakespeare's Hamlet und seine Beurteiler, 1827.

² Fletcher: Westminster Review, Sept., 1845.

³ Klein: Emil Devrient's Hamlet. Berliner Modenspiegel, eine Zeitschrift für die elegante Welt, 1846, Nr. 23, 24.

⁴ Werder: Vorlesungen über Shakespeare's Hamlet, 1875. Translated by E. Wilder, 1907, under the title of "The Heart of Hamlet's Mystery."

for murdering his uncle to seize the throne himself, but also for selfishly seeking to cast an infamous slur on the memory of a man who could no longer defend his honour. This would have resulted in the sanctification of the uncle, and so the frustration of the revenge. In other words it was the difficulty not so much of the act itself that deterred Hamlet as of the situation that would necessarily result from the act.

Thanks mainly to Werder's ingenious presentation of this view, several prominent critics, including Rolfe,¹ Corson,² Furness,³ Hudson⁴ and Halliwell-Phillips⁵ have given it their adherence. It has not found much favour in the Hamlet-literature itself, and has been crushingly refuted by a number of able critics, particularly by Tolman,⁶ Loening,⁷ Hebler,⁸ Ribbeck,⁹ Bradley,¹⁰ Baumgart,¹¹ and Bulthaupt.¹² I need, therefore, do no more than mention one or two of the objections that can be raised to it. It will be seen that to support this hypothesis the task has in two respects been made to appear more difficult than is really the case; first it is assumed to be not a simple revenge in the ordinary sense of the word, but a complicated bringing to judgement in a more or less legal way; and secondly the importance of the external obstacles have been exaggerated. This distortion of the meaning of the revenge is purely gratuitous and has no warrant in any passage of the play, or elsewhere where the word is used in Shakspeare.¹³ Hamlet never doubted that he was the legitimately appointed instrument of punishment, and when at the end of the play he secures his revenge, the dramatic situation is correctly resolved, although the nation is not even informed, let alone convinced, of the murder that is being avenged. To secure evidence that would convict the uncle in a court of law was from the nature of the case impossible, and no tragical situation can arise from an attempt to achieve the impossible, nor can the interest of

¹ Rolfe: Introduction to the English Translation of Werder, 1907.

² Corson: Cited by Rolfe. *Loc. cit.*

³ Furness: A New Var. Ed. of Shakespeare, Vol. III and IV, 1877.

⁴ Hudson: Shakespeare's Life, Art, and Characters, 2nd ed., 1882.

⁵ Halliwell-Phillips: Memoranda on the tragedy of Hamlet, 1879.

⁶ Tolman: Views about Hamlet and Other Essays, 1904.

⁷ Loening: *Op. cit.*, S. 110-113 and 220-224.

⁸ Hebler: Aufsätze über Shakespeare, 2^e Ausg., 1874, S. 258-278.

⁹ Ribbeck: Hamlet und seine Ausleger, 1891, S. 567.

¹⁰ Bradley: Shakespearian Tragedy, 1904, Art. Hamlet.

¹¹ Baumgart: Die Hamlet-Tragödie und ihre Kritik, 1877, S. 7-29.

¹² Bulthaupt: Dramaturgie des Schauspiels, 4^e Aufl., 1891, II, S. 237.

¹³ Loening: (*Op. cit.*, Cap. VI), has made a detailed study of the significance of revenge in Shakspeare's period and as illustrated throughout his works; his conclusion on the point admits of no questioning.

the spectator be aroused for an obviously one-sided struggle. The external situation is similarly distorted for the needs of this hypothesis. On which side the people would have been in any conflict is clearly enough perceived by Claudius, who dare not even punish Hamlet for killing Polonius. (Act IV, Sc. 3.)

"Yet must not we put the strong law on him;
He's loved of the distracted multitude,
Who like not in their judgment, but in their eyes;"

and again in Act IV, Sc. 7,

"The other motive,
Why to a public count I might not go,
Is the great love the general gender bear him;
Who, dipping all his faults in their affection,
Would, like the spring that turneth wood to stone,
Convert his gyves to graces; so that my arrows,
Too slightly timber'd for so loud a wind,
Would have reverted to my bow again,
And not where I had aim'd them."

The ease with which the people could be roused against Claudius is well demonstrated after Polonius' death, when Laertes carried them with him in an irresistible demand for vengeance, which would promptly have been consummated had not the king convinced the avenger that he was innocent. Here the people, the "false Danish dogs" whose loyalty to Claudius was so feather-light that they gladly hailed as king even Laertes, a man who had no sort of claim on the throne, were ready enough to believe in the murderous guilt of their monarch without any shred of supporting evidence, when the accusation was not even true, and where no motive for murder could be discerned at all approaching in weight the two powerful ones that had actually led him to kill his brother. Where Laertes succeeded, it is not likely that Hamlet, the darling of the people, would have failed. Can we not imagine the march of events during the play before the court had Laertes been at the head instead of Hamlet; the straining observation of the fore-warned nobles, the starting-up of the guilty monarch who can bear the spectacle no longer, the open murmuring of the audience, the resistless impeachment by the avenger, and the instant execution effected by him and his devoted friends? Indeed, the whole Laertes episode seems almost to have been purposely woven into the drama so as to shew the world how a pious son should really deal with his father's murderer, how possible was the vengeance under these particular circumstances, and by contrast to illuminate the ignoble vacillation of Hamlet whose honour had been doubly wounded by the same treacherous villain.

Most convincing proof of all that the tragedy cannot be

interpreted as residing in difficulties produced by the external situation is Hamlet's own attitude toward his task. He never behaves as a man confronted with a straight-forward task, in which there are merely external difficulties to overcome. If this had been the case surely he would from the first have confided in Horatio and his other friends who so implicitly believed in him, and would deliberately have set to work with them to formulate plans by means of which these obstacles might be overcome. Instead of this he never makes any serious attempt to deal with the external situation, and indeed throughout the play makes no concrete reference to it as such, even in the significant prayer scene when he had every opportunity for disclosing the reasons for his non-action. There is therefore no escape from the conclusion that so far as the external situation is concerned the task was a possible one.

If Hamlet is a man capable of action, and the task is one capable of achievement, what then can be the reason that he does not execute it? Critics who have realised the inadequacy of the above-mentioned hypotheses have been hard pressed to answer this question. Some, struck by Klein's suggestion that the task is not really what it is generally supposed to be, have offered novel interpretations of it. Thus Mauerhof¹ maintains that the Ghost's command to Hamlet was not to kill the king but to put an end to the life of depravity his mother was still leading, and that Hamlet's problem was how to do this without tarnishing her fair name. Dietrich² put forward the singular view that Hamlet's task was to restore to Fortinbras the lands that had been unjustly filched from the latter's father. When straits such as these are reached it is no wonder that many competent critics have taken refuge in the conclusion that the tragedy is in its essence inexplicable, incongruous and incoherent. This view, first sustained in 1846 by Rapp,³ has been developed by a number of writers, including Rümelin⁴, Benedix⁵, Von Friefen⁶, and many others. The causes of the dramatic imperfection of the play have been variously stated, by Dowden⁷ as a conscious interpolation by Shakspeare of some secret, by Reichel⁸ as the defacement by an

¹ Mauerhof: Ueber Hamlet, 1882.

² Dietrich: Hamlet, der Konstelabel der Vorsehung; eine Shakespear-Studie, 1883.

³ Rapp: Shakespeare's Schauspiele übersetzt und erläutert. Bd. VIII, 1846.

⁴ Rümelin: Shakespeare-Studien, 1866.

⁵ Benedix: Die Shakespearomanie, 1873.

⁶ Von Friefen: Briefe über Shakespeare's Hamlet, 1864.

⁷ Dowden: Shakespeare; his development in his works, 1875.

⁸ Reichel: Shakespeare-Litteratur, 1887.

uneducated actor called Shakspere of a play by an unknown poet called Shakespeare, etc. Many upholders of this conclusion have consoled themselves that in this very obscurity, so characteristic of life in general, lies the power and attractiveness of the play. Even Grillparzer¹ saw in its impenetrability the reason for its colossal effectiveness; he adds "Dadurch wird es zu einem getreuen Bild der Weltbegebenheiten und wirkt ebenso ungeheuer als diese." Now, vagueness and obfuscation may or may not be characteristic of life in general, but they are certainly not the attributes of a successful drama. No disconnected and meaningless drama could have produced the effects on its audiences that Hamlet has continuously done for the past three centuries. The underlying meaning of the drama may be totally obscure, but that there is one, and one which touches on problems of vital interest to the human heart, is empirically demonstrated by the uniform success with which the drama appeals to the most diverse audiences. To hold the contrary is to deny all the canons of dramatic art accepted since the time of Aristotle. Hamlet as a masterpiece stands or falls by these canons.

We are compelled then to take the position that there is some cause for Hamlet's vacillation which has not yet been fathomed. If this lies neither in his incapacity for action in general, nor in the inordinate difficulty of the task in question, then it must of necessity lie in the *third* possibility, namely in some special feature of the task that renders it repugnant to him. This conclusion, that Hamlet at heart does not want to carry out the task, seems so obvious that it is hard to see how any critical reader of the play could avoid making it.² Some of the direct evidence for it furnished in the play will presently be brought forward when we discuss the problem of the cause for his repugnance, but it will first be necessary to mention some of the views that have been expressed on this subject. The first writer clearly to recognise that Hamlet was a man not baffled in his endeavours but struggling in an internal conflict was Ulrici³ in 1839. The details of Ulrici's hypothesis, which like Klein's, originated in the Hegelian views of morality, are hard to follow, but the essence of it is the contention that Hamlet gravely doubted the moral legitimacy of revenge. He was thus plunged in a struggle between his natural tendency to avenge his father and his highly developed ethical

¹ Grillparzer: Studien zur Litterargeschichte, 3^e Ausg., 1880.

² Anyone who doubts this conclusion is recommended to read Loening's convincing chapter (XII), "Hamlet's Verhalten gegen seiner Aufgabe."

³ Ulrici: Shakespeare's dramatische Kunst; Geschichte und Charakteristik des Shakespeare'schen Dramas, 1839.

and Christian views, which forbade the indulging of this instinctive desire. This hypothesis has been much developed of late years, most extensively by Liebau,¹ Mézières,² Gerth,³ Baumgart,⁴ and Robertson,⁵ on moral, ethical and religious lines. Kohler⁶ ingeniously transferred the conflict to the sphere of jurisprudence, maintaining that Hamlet was in advance of his time in recognising the superiority of legal punishment to private revenge, and was thus a fighter in the van of progress. This special pleading has been effectually refuted by Loening⁷ and Fuld,⁸ and is contradicted by all historical considerations. Finally Schipper⁹ and, more recently, Gelber¹⁰ have suggested that the conflict was a purely intellectual one, in that Hamlet was unable to satisfy himself of the adequacy or reliability of the Ghost's evidence.

The obvious question that one puts to the upholders of any of the above hypotheses is: why did Hamlet in his monologues give us no indication of the nature of the conflict in his mind? As we shall presently see, he gave several excuses for his hesitancy, but never once did he hint at any doubt about what his duty was in the matter. He was always clear enough about what he *ought* to do; the conflict in his mind ranged about the question why he could n't bring himself to do it. If Hamlet had at any time been asked whether it was right for him to kill his uncle, or whether he definitely intended to do so, no one can seriously doubt what his instant answer would have been. Throughout the play we see his mind irrevocably made up as to the necessity of a given course of action, which he fully accepts as being his bounden duty; indeed, he would have resented the mere insinuation of doubt on this point as an untrue slur on his filial piety. Ulrici, Baumgart and Kohler try to meet this difficulty by assuming that the ethical objection to personal revenge was never clearly

¹ Liebau: Studien über William Shakespeares Trauerspiel Hamlet. Date not stated.

² Mézières: Shakspeare, ses oeuvres et ses critiques, 1860.

³ Gerth: *Op. cit.*

⁴ Baumgart: *Op. cit.*

⁵ Robertson: Montaigne and Shakspeare, 1897, p. 129.

⁶ Kohler: Shakespeare vor dem Forum der Jurisprudenz, 1883, and Zur Lehre von der Blutrache, 1885. See also Zeitschr. f. vergleichende Rechtswissenschaft, Bd. V, S. 330.

⁷ Loening: Zeitschrift für die gesamte Strafrechtswissenschaft, Bd. V, S. 191.

⁸ Fuld: Shakespeare und die Blutrache. Dramaturgische Blätter und Bühnen-Rundschau, 1888, Nr. 44.

⁹ Schipper: Shakespeare's Hamlet; æsthetische Erläuterung des Hamlet, etc., 1862.

¹⁰ Gelber: Shakespeare'sche Probleme, Plan und Einheit im Hamlet. 1891.

present to Hamlet's mind; it was a deep and undeveloped feeling that had not fully dawned. I would agree that in no other way can the difficulty be logically met, and further, that in the recognition of Hamlet's non-consciousness of the cause of the repugnance to his task we are nearing the core of the mystery. But an invincible difficulty in the way of accepting any of the causes of repugnance suggested above is that the nature of them is such that a keen and introspective thinker, as Hamlet was, would infallibly have recognised them, and would have openly debated them instead of deceiving himself with a number of false pretexts in the way we shall presently mention. Loening¹ well states this in the sentence: "Handelt es sich um einen Konflikt zwischen der von aussen gebotenen Rachepflicht und einer inneren *sittlichen* oder *rechtlichen* Gegenströmung, so *muss* dieser Zwiespalt und seine Ursache bei einem so denkkraftigen und ans Denken gewohnten Menschen wie Hamlet zur Reflexion gebracht werden."

In spite of this difficulty the hint of an approaching solution encourages us to pursue more closely the argument at that point. The hypothesis stated above may be correct up to a certain stage and then have failed for lack of special knowledge to guide it further. Thus Hamlet's hesitancy may have been due to an internal conflict between the need to fulfil his task on the one hand, and some special cause of repugnance to it on the other; further, the explanation of his not disclosing this cause of repugnance may be that he was not conscious of its nature; and yet the cause may be one that doesn't happen to have been considered by any of the upholders of the hypothesis. In other words the first two stages in the argument may be correct, but not the third. This is the view that will now be developed, but before dealing with the third stage in the argument it is first necessary to establish the probability of the first two, namely that Hamlet's hesitancy was due to some special cause of repugnance for his task, and that he was unaware of the nature of this repugnance.

A preliminary obstruction to this line of thought, based on some common prejudices on the subject of mental dynamics, may first be considered. If Hamlet was not aware of the cause of his inhibition, doubt may be felt as to the possibility of our penetrating to it. This pessimistic thought was thus expressed by Baumgart:² "Das was ihn—Hamlet—an der Rache hindert, ist ihm selbst ein Problem und *deshalb* musste es für uns alle ein Problem bleiben." Fortunately for our investigation, however, psycho-analytic study has proved beyond doubt that

¹ Loening: Die Hamlet-Tragödie Shakespeares, 1893, S. 78.

² Baumgart: *Op. cit.* S. 48.

mental trends hidden from the subject himself may come to external expression in a way that reveals their nature to a trained observer, so that the possibility of success is not to be thus excluded. Loening¹ has further objected that the poet himself has not revealed this hidden mental trend, or even given any indication of it. The first part of this objection is certainly true, otherwise there would be no problem to discuss, but we shall presently see that the second is by no means true. It may be asked: why has the poet not put in a clearer light the mental trend we are trying to discover? Strange as it may appear, the answer is the same as in the case of Hamlet himself, namely, he could not, because he was unaware of its nature. We shall later deal with this matter in connection with the relation of the poet to the play. But, if the motive of the play is so obscure, to what can we attribute its powerful effect on the audience, for, as Kohler² asks, "Wer, der je Hamlet gesehen, hat nicht den furchtbaren Konflikt empfunden, welcher die Seele des Helden bewegt?" This can only be because the hero's conflict finds its echo in a similar inner conflict in the mind of the hearer, and the more intense is this already present conflict the greater is the effect of the drama.³ Again, the hearer himself does not know the inner cause of the conflict in his mind, but experiences only the outer manifestations of it. We thus reach the apparent paradox that the hero, the poet, and the audience are all profoundly moved by feelings due to a conflict of the source of which they are unaware.

The fact, however, that such a conclusion should seem paradoxical is in itself a censure on popular views of the actual workings of the human mind, and, before undertaking to sustain the assertions made in the preceding paragraph, it will first be necessary to make a few observations on prevailing views of motive and conduct in general. The new science of clinical psychology stands nowhere in sharper contrast to the older attitudes towards mental functioning than on this very matter. Whereas the generally accepted view of man's mind, usually implicit and frequently explicit in psychological writings, regards it as an interplay of various processes that are for the most part known to the subject, or are at all events accessible to careful introspection on his part, the analytic methods of clinical psychology have on the contrary decisively

¹Loening: *Op. cit.*, S. 78, 79.

²Kohler: *Shakespeare vor dem Forum des Jurisprudenz*, 1883, S. 195.

³It need hardly be said that the play appeals to its audience in a number of different respects. We are here considering only the main appeal, the central conflict in the tragedy.

proved that a far greater number of these processes than is commonly surmised arise from origins that he never suspects. Man's belief that he is a self-conscious animal, alive to the desires that impel or inhibit his actions, and aware of all the springs of his conduct, is the last stronghold of that anthropomorphic outlook on life which so long has dominated his philosophy, his theology and, above all, his psychology. In other words, the tendency to take man at his own valuation is rarely resisted, and we assume that the surest way of finding out why a person does a given thing is simply to ask him, relying on the knowledge that he, like ourselves in a like circumstance, will feel certain of the answer and will infallibly provide a plausible reason for his conduct. Special objective methods of penetrating into obscure mental processes, however, disclose the most formidable obstacles in the way of this direct introspective route, and reveal powers of self-deception in the human mind to which a limit has yet to be found. If I may be allowed to quote from a former paper:¹ "We are beginning to see man not as the smooth, self-acting agent he pretends to be, but as he really is, a creature only dimly conscious of the various influences that mould his thought and action, and blindly resisting with all the means at his command the forces that are making for a higher and fuller consciousness."

That Hamlet is suffering from an internal conflict, the essential nature of which is inaccessible to his introspection, is evidenced by the following considerations. Throughout the play we have the clearest picture of a man who sees his duty plain before him, but who shirks it at every opportunity, and suffers in consequence the most intense remorse. To paraphrase Sir James Paget's famous description of hysterical paralysis: Hamlet's advocates say he cannot do his duty, his detractors say he will not, whereas the truth is that he cannot will. Further than this, the defective will-power is localised to the one question of killing his uncle; it is what may be termed a *specific aboulia*. Now instances of such specific aboulias in real life invariably prove, when analysed, to be due to an unconscious repulsion against the act that cannot be performed. In other words, whenever a person cannot bring himself to do something that every conscious consideration tells him he should do, it is always because for some reason he doesn't want to do it; this reason he will not own to himself and is only dimly if at all aware of. That is exactly the case with Hamlet. Time and again he works himself up,

¹ Rationalisation in Every Day Life. Journal of Abnormal Psychology, Aug.—Sept., 1908, Vol. III, p. 168.

points out to himself his obvious duty, with the cruellest self-reproaches lashes himself to agonies of remorse, and once more falls away into inaction. He eagerly seizes every excuse for occupying himself with any question rather than the performance of his duty, just as on a lesser plane a schoolboy faced with a distasteful task whittles away his time in arranging his books, sharpening his pencils, and fidgeting with any little occupation that will serve as a pretext for putting off the task.

Highly significant is the fact that the grounds Hamlet gives for his hesitancy are grounds none of which will stand a moment's serious consideration, and which continually change from one time to another. One moment he pretends he is too cowardly to perform the deed or that his reason is paralysed by "bestial oblivion," at another he questions the truthfulness of the ghost, in another, when the opportunity presents itself in its naked form, he thinks the time is unsuited,—it would be better to wait till the king was in some evil act and then to kill him, and so on. When a man gives at different times a different reason for his conduct it is safe to infer that, whether purposely or not, he is concealing the true reason. Wetz,¹ discussing a similar problem in reference to Iago, penetratingly observed, "nichts ist ein so guter Beweis für die Unechtheit der Motive, die Iago sich einreden will, *als der stete Wechsel dieser Motive.*" We can therefore safely dismiss all the alleged motives that Hamlet propounds, as being more or less successful attempts on his part to blind himself with self-deception. Loening's² summing-up of them is not too emphatic, when he says, "alle widersprechen sich, *es sind samt und sonders falsche und Scheingründe.*" The more specious the explanation Hamlet puts forth the more easily does it satisfy him, and the more readily will the reader accept it as the real motive. The alleged motives excellently illustrate the mechanisms of psychological evasion and rationalisation I have elsewhere described.³ It is not necessary, however, to discuss them individually, for Loening has with the greatest perspicacity done this in detail, and has effectually demonstrated how utterly untenable they all are.⁴

Still, in his moments of self-reproach Hamlet sees clearly though the recalcitrancy of his conduct, and renews his efforts to achieve action. It is interesting to notice how his out-

¹ Wetz: Shakespeare vom Standpunkt der vergleichenden Literaturgeschichte, 1890, Bd. I, S. 186.

² Loening: *Op. cit.*, S. 245.

³ *Op. cit.*, p. 161.

⁴ See especially his analysis of Hamlet's pretext for non-action in the prayer scene. *Op. cit.*, S. 240-242.

bursts of remorse are evoked by external happenings which bring back to his mind that which he would so gladly forget; particularly effective in this respect are incidents that contrast with his own conduct, as when the player is so moved over the fate of Hecuba (Act II, Sc. 2), or when Fortinbras takes the field and "finds quarrel in a straw when honour's at the stake." (Act IV, Sc. 4.) On the former occasion, stung by the "monstrous" way in which the player pours out his feeling at the thought of Hecuba, he arraigns himself in words which surely should effectually dispose of the view that he has any doubt where his duty lies.

"What's Hecuba to him, or he to Hecuba,
That he should weep for her? What would he do
Had he the motive and the cue for passion
That I have? He would drown the stage with tears,
And cleave the general ear with horrid speech,
Make mad the guilty and appal the free,
Confound the ignorant, and amaze indeed
The very faculties of eyes and ears.
Yet I,
A dull and muddy-mettled rascal, peak,
Like John-a-dreams, unpregnant of my cause,¹
And can say nothing; no, not for a king,
Upon whose property and most dear life
A damn'd defeat was made. Am I a coward?
Who calls me villain? breaks my pate across?
Plucks off my beard, and blows it in my face?
Tweaks me by the nose? gives me the lie i' the throat,
As deep as to the lungs? Who does me this?
Ha!
'Swounds, I should take it; for it cannot be
But I am pigeon-liver'd, and lack gall
To make oppression bitter; or ere this
I should have fatted all the region kites
With this slave's offal; bloody, bawdy villain!
Remorseless, treacherous, lecherous, kindless villain!
O, vengeance!
Why, what an ass am I! This is most brave,
That I, the son of a dear father murder'd,
Prompted to my revenge by heaven and hell,
Must, like a whore, unpack my heart with words,
And fall a-cursing, like a very drab,
A scullion!"

The readiness with which his guilty conscience is stirred into activity is again evidenced on the second appearance of the Ghost when Hamlet cries,

"Do you not come your tardy son to chide,
That, lapsed in time and passion, lets go by
The important acting of your dread command?
Oh, say!"

The Ghost at once confirms this misgiving by answering,

"Do not forget: this visitation
Is but to whet thy almost blunted purpose."

¹ How the very core of the problem is contained in these four words.

In short, the whole picture presented by Hamlet, his deep depression, the hopeless note in his attitude towards the world and towards the value of life, his dread of death,¹ his repeated reference to bad dreams, his self-accusations, his desperate efforts to get away from the thoughts of his duty, and his vain attempts to find an excuse for his recalcitrancy; all this unequivocally points to a tortured conscience, to some hidden ground for shirking his task, a ground which he dare not or cannot avow to himself. We have, therefore, again to take up the argument at this point, and to seek for some evidence that may serve to bring to the light of day the hidden motive.

The extensive experience of the psycho-analytic researches carried out by Freud and his school during the past twenty years has amply demonstrated that certain kinds of mental processes shew a greater tendency to be "repressed" (*verdrängt*) than others. In other words, it is harder for a person to own to himself the existence in his mind of some mental trends than it is of others. In order to gain a correct perspective it is therefore desirable briefly to enquire into the relative frequency with which various sets of mental processes are "repressed." One might in this connection venture the generalisation that those processes are most likely to be "repressed" by the individual which are most disapproved of by the particular circle of society to whose influence he has chiefly been subjected. Biologically stated, this law would run: "That which is unacceptable to the herd becomes unacceptable to the individual unit," it being understood that the term herd is intended in the sense of the particular circle above defined, which is by no means necessarily the community at large. It is for this reason that moral, social, ethical or religious influences are hardly ever "repressed," for as the individual originally received them from his herd, they can never come into conflict with the dicta of the latter. This merely says that a man cannot be ashamed of that which he respects; the apparent exceptions to this need not here be explained. The contrary is equally true, namely that mental trends "repressed" by the individual are those least acceptable to his herd; they are, therefore, those which are, curiously enough, distinguished as "natural" instincts, as contrasted with secondarily acquired mental trends. Loening² seems very discerningly to have grasped this, for, in commenting on a remark of Kohler's to the effect that "Wo uns ein Gefühl zum Handeln oder Unter-

¹Tieck (*Dramaturgische Blätter*, II, 1826) saw in Hamlet's cowardly fear of death a chief reason for his hesitancy in executing his vengeance.

²*Op. cit.*, S. 245, 246.

lassen drängt, da ist es mit hundert Gründen schwanger, mit Gründen, die so leicht sind wie Seifenblasen, aber uns durch Selbstbetrug als höchst respektable, als zwingende Motive erscheinen, weil sie im Hohlspiegel unseres eigenen Gefühls zur riesigen Grösse hinaufgetäuscht werden," he writes "Nur gilt dies nicht, wie Kohler und andere glauben, wenn uns *sittliche*, von der Vernunft *gebilligte* Gefühle drängen (denn diese gestehen wir uns ein, hier bedarf es keines Vorwands), sondern lediglich bei Gefühlen, die aus unserem *Naturell* aufsteigen und deren Befriedigung *der Vernunft widerstreitet*." It only remains to add the obvious corollary that, as the herd unquestionably selects from the "natural" instincts the sexual ones on which to lay its heaviest ban, so is it the various psycho-sexual trends that most often are "repressed" by the individual. We have here an explanation of the clinical experience that the more intense and the more obscure is a given case of deep mental conflict the more certainly will it be found, on adequate analysis, to centre about a sexual problem. On the surface, of course, this does not appear so, for, by means of various psychological defensive mechanisms, the depression, doubt, and other manifestations of the conflict are transferred on to more acceptable subjects, such as the problems of immortality, future of the world, salvation of the soul, and so on.

Bearing these considerations in mind, let us return to Hamlet. It should now be evident that the conflict hypotheses above mentioned, which see Hamlet's "natural" instinct for revenge inhibited by an unconscious misgiving of a highly ethical kind, are based on ignorance of what actually happens in real life, for misgivings of this kind are in fact readily accessible to introspection. Hamlet's self-study would speedily have made him conscious of any such ethical misgivings, and although he might subsequently have ignored them, it would almost certainly have been by the aid of a process of rationalisation which would have enabled him to deceive himself into believing that such misgivings were really ill founded; he would in any case have remained conscious of the nature of them. We must therefore invert these hypotheses, and realise that the positive striving for revenge was to him the moral and social one, and that the suppressed negative striving against revenge arose in some hidden source connected with his more personal, "natural" instincts. The former striving has already been considered, and indeed is manifest in every speech in which Hamlet debates the matter; the second is, from its nature, more obscure and has next to be investigated.

This is perhaps most easily done by inquiring more intently into Hamlet's precise attitude towards the object of his vengeance, Claudius, and towards the crimes that have to be

avenged. These are two, Claudius' incest with the Queen, and his murder of his brother. It is of great importance to note the fundamental difference in Hamlet's attitude towards these two crimes. Intellectually of course he abhors both, but there can be no question as to which arouses in him the deeper loathing. Whereas the murder of his father evokes in him indignation, and a plain recognition of his obvious *duty* to avenge it, his mother's guilty conduct awakes in him the intensest horror. Furnivall¹ well remarks, in speaking of the Queen, "Her disgraceful adultery and incest, and treason to his noble father's memory, Hamlet has felt in his inmost soul. Compared to their ingrain die, Claudius' murder of his father—notwithstanding all his protestations—is only a skin-deep stain." Now, in trying to define Hamlet's attitude towards his uncle we have to guard against assuming offhand that this is a simple one of mere execration, for there is a possibility of complexity arising in the following way: The uncle has not merely committed *each* crime, he has committed *both* crimes, a distinction of considerable importance, for the *combination* of crimes allows the admittance of a new factor, produced by the possible inter-relation of the two, which prevents the result from being simply one of summation. In addition it has to be borne in mind that the perpetrator of the crimes is a relative, and an exceedingly near relative. The possible inter-relation of the crimes, and the fact that the author of them is an actual member of the family on which they were perpetrated, gives scope for a confusion in their influence on Hamlet's mind that may be the cause of the very obscurity we are seeking to clarify.

We must first pursue further the effect on Hamlet of his mother's misconduct. Before he even knows that his father has been murdered he is in the deepest depression, and evidently on account of this misconduct. The connection between the two is unmistakable in the monologue in Act I, Sc. 2, in reference to which Furnivall² writes, "One must insist on this, that before any revelation of his father's murder is made to Hamlet, before any burden of revenging that murder is laid upon him, he thinks of suicide as a welcome means of escape from this fair world of God's, made abominable to his diseased and weak imagination by his mother's lust, and the dishonour done by her to his father's memory."

"O! that this too solid flesh would melt,
Thaw and resolve itself into a dew;
Or that the Everlasting had not fix'd
His canon 'gainst self-slaughter! O God! O God!

¹ Furnivall: Introduction to the "Leopold" Shakespeare, p. 72.

² Furnivall: *Op. cit.*, p. 70.

How weary, stale, flat, and unprofitable
 Seem to me all the uses of this world!
 Fie on 't! O fie! 't is an unweeded garden
 That grows to seed; things rank and gross in nature
 Possess it merely. That it should come to this!
 But two months dead! nay, not so much, not two;
 So excellent a king; that was, to this,
 Hyperion to a satyr; so loving to my mother
 That he might not beteem the winds of heaven
 Visit her face too roughly. Heaven and earth!
 Must I remember? why, she would hang on him,
 As if increase of appetite had grown
 By what it fed on; and yet, within a month—
 Let me not think on't.—Frailty, thy name is woman!
 A little month! or ere those shoes were old
 With which she follow'd my poor father's body,
 Like Niobe, all tears; why she, even she—
 O God! a beast, that wants discourse of reason,
 Would have mourn'd longer,—married with my uncle,
 My father's brother, but no more like my father
 Than I to Hercules. Within a month?
 Ere yet the salt of most unrighteous tears
 Had left the flushing in her galled eyes,
 She married. O, most wicked speed, to post
 With such dexterity to incestuous sheets!
 It is not nor it cannot come to good;
 But break my heart, for I must hold my tongue!"

But we can rest satisfied that this seemingly adequate explanation of Hamlet's weariness of life is a complete one only if we unquestionably accept the conventional standards of the causes of deep emotion. The very fact that Hamlet is content with the explanation arouses our gravest suspicions, for, as will presently be explained, from the very nature of the emotion he cannot be aware of the true cause of it. If we ask, not what *ought* to produce such soul-paralysing grief and distaste for life, but what in actual fact *does* produce it, we must go beyond this explanation and seek for some deeper cause. In real life speedy second marriages occur commonly enough without leading to any such result as is here depicted, and when we see them followed by this result we invariably find, if the opportunity for an analysis of the subject's mind presents itself, that there is some other and more hidden reason why the event is followed by this inordinately great effect. The reason always is that the event has awakened to increased activity mental processes that have been "repressed" from the subject's consciousness. His mind has been prepared for the catastrophe by previous mental processes, with which those directly resulting from the event have entered into association. This is perhaps what Furnivall means when he speaks of the world being made abominable to Hamlet's "diseased imagination." Further, to those who have devoted much time to the study of such conditions the self-description given here by

Hamlet will be recognised as a wonderfully accurate picture of a particular mental state that is often loosely and incorrectly classified under the name of "neurasthenia."¹ Analysis of such states always reveals the operative activity of some forgotten group of mental processes, which on account of their unacceptable nature have been "repressed" from the subject's consciousness. Therefore if Hamlet has been plunged into this abnormal state by the news of his mother's second marriage it must be because the news has awakened into activity some slumbering memory, which is so painful that it may not become conscious.

For some deep-seated reason, which is to him unacceptable, Hamlet is plunged into anguish at the thought of his father being replaced in his mother's affection by some one else. It is as though his devotion to his mother had made him so jealous for her affection that he had found it hard enough to share this even with his father, and could not endure to share it with still another man. Against this thought, suggestive as it is, may be urged three objections. First, if it were in itself a full statement of the case, Hamlet would easily have become aware of the jealousy, whereas we have concluded that the mental process we are seeking is hidden from him; secondly, we see in it no evidence of the arousing of an old and forgotten memory; and thirdly, Hamlet is being deprived by Claudius of no greater share of the Queen's affection than he had been by his own father, for the two brothers made exactly similar claims in this respect, namely those of a loved husband. The last-named objection, however, has led us to the heart of the situation. How if, in fact, Hamlet had in years gone by bitterly resented having to share his mother's affection even with his father, had regarded him as a rival, and had secretly wished him out of the way so that he might enjoy undisputed the monopoly of that affection? If such thoughts had been present to him in his child days they evidently would have been gradually suppressed, and all traces of them obliterated, by filial piety and other educative influences. The actual realisation of his early wish in the death of his father would then have stimulated into activity these suppressed memories, which would have produced, in the form of depression and other suffering, an obscure aftermath of his childhood's conflict.

I am aware that to those Shaksperian critics, who have enjoyed no special opportunities for penetrating into the obscurer sides of mental activities, and who base their views of human motive on the surface valuation given by the agents them-

¹ Hamlet's state of mind more accurately corresponds, as Freud has pointed out, with that characteristic of a certain form of hysteria.

selves—to whom all conduct whether good or evil at all events springs from conscious sources,—are likely to regard the suggestions put forward above as merely constituting one more of the extravagant and fanciful hypotheses of which the Hamlet literature in particular is so full. For the sake, however, of those who may be interested to apprehend the point of view from which this strange hypothesis seems probable I feel constrained to interpolate a few considerations on two matters that are not commonly appreciated, namely a child's feelings of jealousy and his ideas on death.

The whole subject of jealousy in children is one which arouses such prejudice that even well-known facts are either ignored or are not estimated at their true significance. Stanley Hall¹ in his encyclopædic treatise makes a number of very just remarks on the importance of the subject in adolescents, but implies that before the age of puberty this passion is of relatively little consequence. The close relation between jealousy and the desire for the removal of a rival by death, as well as the common process of suppression of these feelings, is clearly illustrated in a remark of his to the effect that: "Many a noble and even great man has confessed that mingled with profound grief for the death and misfortune of their best friends, they were often appalled to find a vein of secret joy and satisfaction, as if their own sphere were larger or better." A similar thought is more openly expressed by Bernard Shaw² when he makes Don Juan, in the Hell Scene, remark: "You may remember that on earth—though of course we never confessed it—the death of any one we knew, even those we liked best, was always mingled with a certain satisfaction at being finally done with them." Such cynicism in the adult is exceeded to an incomparable extent by that of the child with its notorious, and to the parents often heartbreaking, egotism, with its undeveloped social instincts and with its ignorance of the dread significance of death. A child unreasoningly interprets the various encroachments on its privileges, and the obstacles interposed to the immediate gratification of its desires, as meaningless cruelty, and the more imperative is the desire that has been thwarted the more pronounced is the hostility towards the agent of this cruelty. For a reason that will presently be mentioned, the most important encroachment in this respect, and the most frequent, is that made on the child's desire for affection. This hostility is very often seen on the occasion of the birth of a subsequent child, and is usually regarded with amusement as an added contribution to the general

¹ Stanley Hall: *Adolescence*, 1908, Vol. I, p. 358.

² Bernard Shaw: *Man and Superman*, 1903, p. 94.

gaiety called forth by the happy event. When a child, on being told that the doctor has brought him another playfellow, responds with the cry "Tell him to take it away again," he intends this, not, as is commonly believed, as a joke for the entertainment of his elders, but as an earnest expression of his intuition that in future he will have to renounce his previously unquestioned pre-eminence in the family circle, a matter that to him is serious enough.

The second matter, on which there is also much misunderstanding, is that of the attitude of a child towards the subject of death, it being commonly assumed that this is necessarily the same as that of an adult. When a child first hears of any one's death, the only part of its meaning that he realises is that the person is *no longer there*,¹ a consummation which in many cases he fervently desires. It is only gradually that the more dread implications of the phenomenon are borne in upon him. When, therefore, a child expresses the wish that a given person, even a near relative, would die, our feelings would not be so shocked as in fact they are, were we to interpret this wish from the point of view of the child. The same remark applies to the frequent dreams of adults in which the death of a near and dear relative takes place, for the wish here expressed is in most cases a long forgotten one, and one no longer directly operative.

Of the infantile jealousies the one with which we are here occupied is that experienced by a boy towards his father. The precise form of early relationship between child and father is in general a matter of vast importance in both sexes, and plays a predominating part in the future development of the child's character; this theme has been brilliantly expounded by Jung² in a recent essay. The only point that at present concerns us is the resentment felt by a boy towards his father when the latter disturbs his enjoyment of his mother's affection. This feeling, which occurs frequently enough, is the deepest source of the world-old conflict between father and son, between the young and old, the favourite theme of so many poets and writers. The fundamental importance that this conflict, and the accompanying breaking away of the child from the authority of his parents, has both for the individual and for society is clearly stated in the following passage of Freud's:³ "Die Ablösung des heranwachsenden Individuums von der Autorität der Elt-

¹ See Freud: *Traumdeutung*, 1900, S. 175.

² Jung: *Die Bedeutung des Vaters für das Schicksal des Einzelnen*. Jahrbuch f. psychoanalytische u. psychopathologische Forschungen. 1909, Bd. I, I^e Hälfte.

³ Personal communication quoted by Rank, *Der Mythos von der Geburt des Helden*, 1909, S. 64.

ern ist eine der notwendigsten, aber auch schmerzlichsten Leistungen der Entwicklung. Es ist durchaus notwendig, dass sie sich vollziehe, und man darf annehmen, jeder normal gewordene Mensch habe sie in einem gewissen Mass zu Stande gebracht. Ja, der Fortschritt der Gesellschaft beruht überhaupt auf dieser Gegensätzlichkeit der beiden Generationen." That it rests at bottom on sexual grounds was first demonstrated by Freud,¹ when dealing with the subject of the earliest manifestations of the sexual instinct in children. He has shewn² that this instinct does not, as is generally supposed, differ from other biological functions by suddenly leaping into being at the age of puberty in all its full and developed activity, but that like other functions it undergoes a gradual evolution and only slowly attains the form in which we know it in the adult. In other words a child has to learn how to love just as it has to learn how to run, although the former function is so much more intricate and delicate in its adjustment than the latter that the development of it is a correspondingly slower and more involved process. The earliest sexual manifestations are so palpably non-adapted to what is generally considered the ultimate aim and object of the function, and are so general and tentative in contrast to the relative precision of the later manifestations, that the sexual nature of them is commonly not recognised at all. This theme, important as it is, cannot be further pursued here, but it must be mentioned how frequently these earliest dim awakenings are evoked by the intimate physical relations existing between the child and the persons of his immediate environment, above all, therefore, his parents. As Freud has put it, "The mother is the first seductress of her boy." There is a great variability in both the date and the intensity of the early sexual manifestations, a fact that depends partly on the boy's constitution and partly on the mother's. When the attraction exercised by the mother is excessive it may exert a controlling influence over the boy's later destiny. Of the various results that may be caused by the complicated interaction between this and other influences only one or two need be mentioned. If the awakened passion undergoes but little "repression"—an event most frequent when the mother is a widow—then the boy may remain throughout life abnormally attached to his mother and unable to love any other woman, a not uncommon cause of bachelorhood. He may be gradually weaned from this attachment, if it is less strong, though it often happens

¹ Freud: *Traumdeutung*, 1900, S. 176-180. He has strikingly illustrated the subject in a recent detailed study, "Analyse der Phobie eines fünfjährigen Knaben." *Jahrbuch f. psychoanalytische u. psychopathologische Forschungen*, 1909, Bd. I, I. Hälfte.

² Freud: *Drei Abhandlungen zur Sexualtheorie*, 1905.

that the weaning is incomplete so that he is able to fall in love only with women that resemble the mother; the latter occurrence is a frequent cause of marriage between relatives, as has been interestingly pointed out by Abraham.¹ The maternal influence may also manifest itself by imparting a strikingly tender feminine side to the later character.² When the aroused feeling is intensely "repressed," and associated with shame, guilt, etc., the memory of it may be so completely submerged that it becomes impossible not only to revive it but even to experience any similar feeling, *i. e.*, of attraction for the opposite sex. This may declare itself in pronounced misogyny, or even, when combined with other factors, in actual homosexuality, as Sadger³ has shewn.

The attitude towards the successful rival, namely the father, also varies with the extent to which the aroused feelings have been "repressed." If this is only slight, then the natural resentment against the father may later on be more or less openly manifested, a rebellion which occurs commonly enough, though the original source of it is not recognised. To this source many social revolutionaries owe the original impetus of their rebellion against authority, as can often be plainly traced—for instance in Shelley's case. If the "repression" is more intense, then the hostility towards the father is also concealed; this is usually brought about by the development of the opposite sentiment, namely of an exaggerated regard and respect for him, and a morbid solicitude for his welfare, which completely cover the true underlying relation. The illustration of the attitude of son to parent is so transpicuous in the *Œdipus* legend,⁴ as developed for instance in Sophocles' tragedy, that the group of mental processes concerned is generally known under the name of the "*Œdipus-complex*."

We are now in a position to expand and complete the sug-

¹Abraham: *Verwandtenehe und Neurose*. Berl. Gesell. f. Psychiatr. u. Nervenkrankh, Nov. 8, 1908. *Neurolog. Centralbl.*, 1908, S. 1150.

²This trait in Hamlet's character has often been the subject of comment. See especially Bodenstedt, *Hamlet*. Westermann's illustrierte Monatshefte, 1865; we mentioned above Vining's suggestion that Hamlet was really a woman. That the same trait was prominent in Shakspeare himself is well known, a fact which the appellation of "gentle Will" sufficiently recalls.

³Sadger: *Fragment der Psychoanalyse eines Homosexuellen*. Jahrbuch f. sex. Zwischenstufen, 1908, Bd. IX. Ist die Konträre Sexualempfindung heilbar? *Zeitschr. f. Sexualwissenschaft*, Dez., 1908. Zur Ätiologie der konträren Sexualempfindung. *Mediz. Klinik*, 1909. Nr. 2.

⁴See Freud: *Traumdeutung*, 1900, S. 181. Interesting expositions of the mythological aspects of the subject are given by Abraham, *Traum und Mythos*, 1909, and Rank, *Op. cit.*

gestions offered above in connection with the Hamlet problem.¹ The story thus interpreted would run somewhat as follows: As a child Hamlet had experienced the warmest affection for his mother, and this, as is always the case, had contained elements of a more or less dimly defined erotic quality. The presence of two traits in the Queen's character go to corroborate this assumption, namely her markedly sensual nature, and her passionate fondness for her son. The former is indicated in too many places in the play to need specific reference, and is generally recognised. The latter is equally manifest; as Claudius says (Act IV, Sc. 7, l. 11), "The Queen his mother lives almost by his looks." Hamlet seems, however, to have with more or less success weaned himself from her, and to have fallen in love with Ophelia. The precise nature of his original feeling for Ophelia is a little obscure. We may assume that at least in part it was composed of a normal love for a prospective bride, but there are indications that even here the influence of the old attraction for his mother is still exerting itself. Although some writers, following Goethe,² see in Ophelia many traits of resemblance to the Queen, surely more striking are the traits contrasting with those of the Queen. Whatever truth there may be in the many German conceptions of Ophelia as a sensual wanton³—misconceptions that have been adequately disproved by Loening⁴ and others—still the very fact that it needed what Goethe happily called the "innocence of insanity" to reveal the presence of any such libidinous thoughts in itself demonstrates the modesty and chasteness of her habitual demeanour. Her naïve piety, her obedient resignation and her unreflecting simplicity sharply contrast with the Queen's character, and seem to indicate that Hamlet by a characteristic reaction towards the opposite extreme had unknowingly been impelled to choose a woman who would least remind him of his mother. A case might

¹ Here, as throughout this essay, I closely follow Freud's interpretation given in the footnote previously referred to. He there points out the inadequacy of the earlier explanations, deals with Hamlet's feelings toward his mother, father and uncle, and mentions two other matters that will presently be discussed, the significance of Hamlet's reaction against Ophelia and of the fact that the play was written immediately after the death of Shakspeare's father.

² Goethe: *Wilhelm Meister*, IV, 14. "Ihr ganzes Wesen schwebt in reifer, süsser Sinnlichkeit." "Ihre Einbildungskraft ist angesteckt, ihre stille Bescheidenheit atmet eine liebevolle Begierde, und sollte die bequeme Göttin Gelegenheit das Bäumschen schütteln, so würde die Frucht sogleich herabfallen."

³ Storffrich: *Psychologische Aufschüsse über Shakespeare's Hamlet*, 1859, S. 131; Dietrich, *Op. cit.*, S. 129; Tieck: *Dramaturgische Blätter*, II, S. 85, etc.

⁴ Loening: *Op. cit.*, Cap. XIII. Charakter und Liebe Ophelias.

further be made out for the view that part of Hamlet's courtship of Ophelia originated not so much in direct attraction for her as in a half-conscious desire to play her off against his mother, just as a disappointed and piqued lover is so often thrown into the arms of a more willing rival. When in the play scene he replies to his mother's request to sit by her with the words, "No, good mother, here's metal more attractive," and proceeds to lie at Ophelia's feet, we seem to have a direct indication of this attitude, and his coarse familiarity and bandying of ambiguous jests with the woman he has recently so ruthlessly jilted are hardly intelligible unless we bear in mind that they were carried out under the heedful gaze of the Queen. It is as though Hamlet is unconsciously expressing to her the following thought: "You give yourself to other men whom you prefer to me. Let me assure you that I can dispense with your favours, and indeed prefer those of a different type of woman."

Now comes the father's death and the mother's second marriage. The long "repressed" desire to take his father's place in his mother's affection is stimulated to unconscious activity by the sight of some one usurping this place exactly as he himself had once longed to do. More, this someone was a member of the same family, so that the actual usurpation further resembled the imaginary one in being incestuous. Without his being at all aware of it these ancient desires are ringing in his mind, are once more struggling to find expression, and need such an expenditure of energy again to "repress" them that he is reduced to the deplorable mental state he himself so vividly depicts. Then comes the Ghost's announcement of the murder. Hamlet, having at the moment his mind filled with natural indignation at the news, answers with (Act I. Sc. 5. l. 29.),

"Haste me to know't, that I, with wings as swift
As meditation or the thoughts of love,
May sweep to my revenge."

The momentous words follow revealing who was the guilty person, namely a relative who had committed the deed at the bidding of lust.¹ Hamlet's second guilty wish had thus also been realised by his uncle, namely to procure the fulfilment of the first—the replacement of his father—by a personal deed, in fact by murder.² The two recent events, the father's

¹ It is not maintained that this was by any means Claudius' whole motive, but it evidently was a powerful one, and the one that most impressed Hamlet.

² Such murderous thoughts, directed against rival members of the same family, are surprisingly common in children, though of course it is relatively rare that they come to expression. Some years ago, in

death and the mother's second marriage, seemed to the world not to be causally related to each other, but they represented ideas which in Hamlet's unconscious fantasy had for many years been closely associated. These ideas now in a moment forced their way to conscious recognition in spite of all "repressing" forces, and found immediate expression in his almost reflex cry: "O my prophetic soul! My uncle?" For the rest of the interview Hamlet is stunned by the effect of the internal conflict in his mind, which from now on never ceases, and into the nature of which he never penetrates.

One of the first manifestations of the awakening in Hamlet's mind of the old conflict is the reaction against Ophelia. This is doubly conditioned, first by his reaction against woman in general, which culminates in the bitter misogyny of his outburst against her,¹ and secondly by the hypocritical prudishness with which Ophelia follows her father and brother in seeing evil in his natural affection, and which poisons his love in exactly the same way that the love of his childhood had been poisoned. On only one occasion does he for a moment escape from the sordid implication with which his love has been impregnated, and achieve a healthier attitude towards Ophelia, namely at the open grave when in remorse he breaks out at Laertes for presuming to pretend that his feeling for Ophelia could ever equal that of her lover. The intensity of the previous repulsion against woman in general, and Ophelia in particular, is an index of the powerful "repression" to which his sexual feeling is being subjected. The outlet for that feeling in the direction of his mother has always been firmly dammed by the forces making for "repression," and, now that the thin outlet for it in Ophelia's direction has also been closed, the increase of desire in the original direction consequent on the awakening of early memories tasks all his energy to maintain the "repression."

It will be seen from the foregoing that Hamlet's attitude towards his uncle is far more complex than is generally supposed. He of course detests his uncle, but it is the jealous detestation of one evil-doer towards his successful fellow.

two editorial articles entitled "Infant Murderers" in the *Brit. Jour. of Children's Diseases* (Nov., 1904, p. 510, and June, 1905, p. 270), I collected a series of such cases, and, mentioning the constant occurrence of jealousy between young children in the same family, pointed out the possible dangers arising from the non-realisation by children of the significance of death.

¹ Act III, Sc. I, l. 149: "I have heard of your paintings too, well enough; God has given you one face, and you make yourselves another; you jig, you amble, and you lisp, and nickname God's creatures, and make your wantonness your ignorance. Go to, I'll no more on't; it hath made me mad."

Much as he hates him, he can never denounce him with the ardent indignation that boils straight from his blood when he reproaches his mother, for the more vigorously he denounces his uncle the more powerfully does he stimulate to activity his own unconscious and "repressed" complexes. He is therefore in a dilemma between on the one hand allowing his natural detestation of his uncle to have free play, a consummation which would make him aware of his own horrible wishes, and on the other ignoring the imperative call for vengeance that his obvious duty demands. He must either realise his own evil in denouncing his uncle's, or strive to ignore, to condone and if possible even to forget the latter in continuing to "repress" the former; his moral fate is bound up with his uncle's for good or ill. The call of duty to slay his uncle cannot be obeyed because it links itself with the call of his nature to slay his mother's husband, whether this is the first or the second; the latter call is strongly "repressed," and therefore necessarily the former also. It is no mere chance that he says of himself that he is prompted to the revenge "by heaven and hell," though the true significance of the expression of course quite escapes him.

Hamlet's dammed-up feeling finds a partial vent in other directions, the natural one being blocked. The petulant irascibility and explosive outbursts called forth by the vexation of Guildenstern and Rosencrantz, and especially of Polonius, are evidently to be interpreted in this way, as also is in part the burning nature of his reproaches to his mother. Indeed towards the end of the interview with his mother the thought of her misconduct expresses itself in that almost physical disgust which is so often the manifestation of intensely "repressed" sexual feeling.

"Let the bloat king tempt you again to bed;
Pinch wanton on your cheek; call you his mouse;
And let him, for a pair of reechy kisses,
Or paddling in your neck with his damn'd fingers,
Make you to ravel all this matter out."

His attitude towards Polonius is highly instructive. Here the absence of family tie, and of other influences, enables him to indulge to a relatively unrestrained degree his hostility towards the prating and sententious dotard. The analogy he effects between Polonius and Jephthah¹ is in this connection especially pointed. It is here that we see his fundamental attitude towards moralising elders who use their power to

¹ What Shakspeare thought of Jephthah's behaviour towards his daughter may be gathered from a reference in Henry VI, Part III, Act V, Sc. 1. See also on the subject Wordsworth. On Shakespeare's knowledge and use of the Bible, 1864, p. 67.

thwart the happiness of the young, and not in the over-drawn and melodramatic portrait in which he delineates his father: "A combination and a form indeed, where every god did seem to set his seal to give the world assurance of a man."

In this discussion of the motives that move or restrain Hamlet we have purposely depreciated the subsidiary ones, which also play a part, so as to bring out in greater relief the deeper and effective ones that are of preponderating importance. These, as we have seen, spring from sources of which Hamlet is unaware, and we might summarise the internal conflict of which he is a victim as consisting in a struggle of the "repressed" mental processes to become conscious. The call of duty, which automatically arouses to activity these unconscious processes, conflicts with the necessity for "repressing" then still further; for the more urgent is the need for external action the greater is the effort demanded of the "repressing" forces. Action is paralysed at its very inception, and there is thus produced the picture of causeless inhibition which is so inexplicable both to Hamlet¹ and to readers of the play. This paralysis arises, however, not from physical or moral cowardice, but from that intellectual cowardice, that reluctance to dare the exploration of his inner mind, which Hamlet shares with the rest of the human race.

We have finally to return to the subject with which we started, namely poetic creation, and in this connection to enquire into the relation of Hamlet's conflict to the inner workings of Shakspeare's mind. It is here maintained that this conflict is an echo of a similar one in Shakspeare himself,² as to a greater or less extent it is in all men. It is, therefore, as much beside the point to enquire into Shakspeare's conscious intention, moral or otherwise, in the play as it is in the case of most works of genius. The play is the form in which his

¹ The situation is perfectly depicted by Hamlet in his cry (Act IV, Sc. 4.):

"I do not know
Why yet I live to say 'This thing's to do,'
Sith I have cause, and will, and strength, and means,
To do't."

With greater insight he could have replaced the word "will" by "pious wish," which, as Loening (*Op. cit.*, S. 246) points out, it obviously means. Curiously enough, Rolfe (*Op. cit.*, p. 23) quotes this very passage in support of Werder's hypothesis that Hamlet was inhibited by the external difficulties of the situation.

² The view that Shakspeare depicted in Hamlet his own inner self is a wide-spread one. See especially Döring, *Shakespeare's Hamlet seinem Grundgedanken und Inhalte nach erläutert*, 1865; Hermann, *Ergänzungen und Berichtigungen der hergebrachten Shakespeare-Biographie*, 1884; Taine, *Histoire de la littérature anglaise*; Vischer, *Altes und Neues*, 1882, Ht. 3.

feeling finds its spontaneous expression, without any inquiry being possible on his part as to the essential nature or source of that feeling.

This conclusion is amply supported by a historical study of the external circumstances of the play. It is well known that Shakspeare took not only the skeleton but also a surprising amount of detail from earlier writings.¹ It is probable that he had read both the original saga as told early in the thirteenth century by Saxo Grammaticus, and the translation and modification of this published by Belleforest.² For at least a dozen years before Shakspeare wrote Hamlet a play of the same name was extant in England, which modern evidence³ has clearly shewn to have been written by Thomas Kyd. Ruder accounts of the story, of Irish and Norse origin, were probably still more widely spread in England, and the name Hamlet itself, or some modification of it, was very common in the Stratford district;⁴ as is well known, Shakspeare in 1585 christened his own son Hamnet, a frequent variation of the name. Thus the plot of the tragedy must have been present in his mind for some years before it actually took form as a play. In all probability this was in the winter of 1601-2, for the play was registered on July 26, 1602, and the first, piratical, edition appeared in quarto in 1603. Highly suggestive, therefore, of the subjective origin of the psychical conflict in the play is the fact that it was in September, 1601, that Shakspeare's father died, an event which might well have had the same awakening effect on old "repressed" memories that the death of Hamlet's father had with Hamlet; his mother lived till some seven years later. There are many indications that the disposition of Shakspeare's father was of that masterful and authoritative kind so apt to provoke rebellion, particularly in a first-born son.

¹ No doubt much detail was also introduced by Shakspeare from personal experience. For instance there is much evidence to shew that in painting the character of Hamlet he had in mind some of his contemporaries, notably William Herbert, later Lord Pembroke, (Döring, *Hamlet*, 1898, S. 35) and Robert Essex (Isaac, *Hamlet's Familie*. *Shakespeare's Jahrbuch*, Bd. XVI, S. 274). The repeated allusion to the danger of Ophelia's conceiving illegitimately may be connected with both Herbert, who was imprisoned for being the father of an illegitimate child, and the poet himself, who hastily married in order to avoid the same stigma.

² Belleforest: *Histoires tragiques*, T. V., 1564. This translation was made from the Italian of Bandello.

³ See Fleay: *Chronicle of the English Drama*, 1891; Sarrazin: *Thomas Kyd und sein Kreis*, 1892; and Corbin: *The Elizabethan Hamlet*, 1895.

⁴ Elton: *William Shakespeare. His Family and Friends*, 1904, p. 223.

It is for two reasons desirable here to interpolate a short account of the mythological relations of the original Hamlet legend, first so as to observe the personal contribution to it made by Shakspeare, and secondly because knowledge of it serves to confirm and expand the psychological interpretation given above. Up to the present point in this essay an attempt has been made to drive the argument along a dry, logical path, and to shew that all the explanations of the mystery prior to Freud's end in blind alleys. So far as I can see, there is no escape possible from the conclusion that the cause of Hamlet's hesitancy lies in some unconscious source of repugnance to his task; the next step of the argument, however, in which is supplied a motive for this repugnance, is avowedly based on considerations that are not generally appreciated, though I have tried to minimise the difficulty by assimilating the argument to some commonly accepted facts. Now, there is another point of view from which this labour would have been superfluous, in that Freud's explanation would appear directly obvious. To any one familiar with the modern interpretation, based on psycho-analytic study, of myths and legends, that explanation of the Hamlet problem would immediately occur on the first reading through of the play. The reason why this strong statement can be made is that the story of Hamlet is merely an unusually elaborated form of a vast group of legends, the psychological significance of which is now, thanks to Freud and his co-workers, quite plain. It would absorb too much space to discuss in detail the historical relationship of the Hamlet legend to the other members of this group, and I shall here content myself with pointing out the psychological resemblances; Jiriczek¹ and Lessmann² have adduced much evidence to shew that the Norse and Irish variants of it are descended from the ancient Iranian legend of Kaikhosrav, and there is no doubt of the antiquity of the whole group, some members of which can be traced back for several thousand years.³

The theme common to all the members of the group is the success of a young hero in displacing a rival father. In its simplest form the hero is persecuted by a tyrannical father who has been warned of his approaching eclipse, but after marvelously escaping from various dangers he avenges himself, often

¹ Jiriczek: *Hamlet in Iran.*, Zeitschr. des Vereins für Volkskunde, 1900, Bd. X.

² Lessman: *Die Kyrossage in Europa.* Wissenschaftliche Beil. z. Jahresbericht d. städt. Realschule zu Charlottenburg, 1906.

³ In the exposition of this group of myths I am especially indebted to Otto Rank's excellent volume, *Der Mythos von der Geburt des Helden*, 1909, in which the original references may also be found.

unwittingly, by slaying the father. The persecution mainly takes the form of attempts to destroy the hero's life just after his birth, by orders that he is to be drowned, exposed to cold and starvation, or otherwise done away with. A good instance of this simple form is the Œdipus legend, in which the underlying motive is betrayed by the hero subsequently marrying his mother; the same occurs in the many Christian variants of this legend, for example, in the Judas Iscariot and St. Gregory ones. The intimate relation of the hero to the mother is also shewn in certain types of the legend (for example, the Ferdun, Perseus and Telephos ones) by the fact that the mother and son are together exposed to the same dangers. In some types the hostility towards the father is the predominating theme, in others the affection for the mother, but as a rule both of these are more or less plainly to be traced.

The elaboration of the more complex variants of the myth is brought about chiefly by three factors, namely: an increasing degree of distortion engendered by greater psychological "repression," complication of the main theme by other allied ones, and expansion of the story by repetition due to the creator's decorative fancy. In giving a description of these three processes it is difficult sharply to separate them, but they will all be illustrated in the following examples.

The *first* disturbing factor, that of more pronounced "repression," manifests itself by the same mechanisms that Freud has described in connection with normal dreams,¹ psychoneurotic symptoms, etc. The most interesting of these mechanisms in myth formation is that of "decomposition" (*Auseinanderlegung*), which is the opposite to the "condensation" (*Verdichtung*) mechanism so characteristic of normal dreams. Whereas in the latter process attributes of several individuals are fused in the creation of one figure, much as in the production of a composite photograph, in the former process various attributes of a given person are disunited and several individuals are invented, each endowed with one group of the original attributes. In this way one person, of complex character, gets replaced by several, each of whom possesses a different aspect of the character that in a simpler form of the myth was combined in one being; usually the different individuals closely resemble one another in other respects, for instance in age. A good example of this process is seen by the figure of the tyrannical father becoming split into two, a father and a tyrant. The resolution of the original figure is most often incomplete, so that the two resulting ones stand in a close relation to each other, being indeed as a rule members of the same

¹ See Abraham: Traum und Mythos, 1908.

family. The tyrant who seeks to destroy the hero is then most commonly the grandfather, as in the legends of Cyrus, Gilgam, Perseus, Telephos and others, or the grand-uncle, as in those of Romulus and Remus and their Greek predecessors, Amphion and Zethos; less often is he the uncle, as in the Hamlet legend. When the decomposition is more complete, the tyrant is not of the same family as the father, though he may be socially related, as in the case of Abraham whose father Therachs was the tyrant Nimrod's commander-in-chief; as a rule the tyrant is in this sub-group a stranger, as in the cases of Moses and Pharaoh, Feridun and Zohäk, Jesus and Herod, and others. In the last two instances, and in many others, not only are the mother and son, but also the father, persecuted by the tyrant, and we thus reach a still more complex variant, well represented by the Feridun legend, in which the son adores his father and avenges him by slaying their common enemy. The picture of the son as avenger instead of as slayer of the father therefore illustrates the highest degree of psychological "repression," in which the true meaning of the story is concealed by the identical mechanism that in real life conceals "repressed" hostility and jealousy in so many families, namely, exaggerated solicitude, care and respect. The dutiful Laertes avenging his murdered father Polonius is probably also an instance of the same stage in the development of the myth. Suppressed hate towards a father would seem to be adequately concealed by being thus masked by devotion and desire to avenge, and Shakspeare's modification of the Hamlet legend is the only instance in which intense "repression" has produced still further distortion of the hero's attitude; in this legend, however, the matter is more complicated by the unusual prominence of the love for the mother over the hate for the father, and by the appearance of other factors such as the relationship of the tyrant to the father and to the mother.

Not only may the two above-mentioned attributes of the parent, fatherliness and tyranny, be split off so as to give rise to the creation of separate figures, but others also. For instance, the power and authority of the parent may be invested in the person of a king or other distinguished man, who may be contrasted with the lowly-born father.¹ In the present legend I think it probable that the figure of Polonius may be thus regarded as resulting from "decomposition" of the parental archetype, and as representing a certain group of qualities which the young not infrequently find an irritating feature in

¹This important theme, which is fully dealt with by Freud and Rank, I have not here discussed, for it does not enter into the present legend. Abraham (*Op. cit.*, S. 40) has interestingly pointed out the significance of it in the development of paranoiac delusions.

their elders. The senile nonentity, concealed behind a show of fussy pomposity, who has developed a rare capacity to bore his audience with the repetition of sententious platitudes in which profound ignorance of life is but thinly disguised by a would-be worldly-wise air; the prying busybody whose meddling is, as usual, excused by his "well-meaning" intentions, constitutes a figure that is sympathetic only to those who submissively accept the world's estimate concerning the superiority of the merely decrepit.

The *second* disturbing factor is that due to the interweaving of the main theme of jealousy and incest between parent and child with other allied ones of a similar kind. We noted above that in the simplest form of decomposition of the paternal attributes the tyrannical rôle is most often relegated to the grandfather. It is no mere chance that this is so, and it is not fully to be accounted for by incompleteness of the decomposition. There is a deeper reason why the grandfather is most often chosen to play the part of tyrant, and this will be readily perceived when we recollect the large number of legends in which he has previously interposed all manner of obstacles to the marriage of his daughter. He opposes the advances of the would-be suitor, sets in his way various apparently impossible tasks and conditions—usually these are miraculously carried out by the lover,—and even locks up his daughter in an inaccessible spot, as in the legends of Gilgames, Perseus, Romulus, Telephos and others. The motive is at bottom that he grudges to give up his daughter to another man, not wishing to part with her himself (Father-daughter complex). When his commands are disobeyed or circumvented, his love for his daughter turns to bitterness, and he pursues her and her offspring with insatiable hate. We are here once more reminded of events that may be observed in daily life by those who open their eyes to the facts. When the grandson in the myth avenges himself and his mother by slaying her tyrannical father, it is as though he clearly realised the motive of the persecution, for in truth he slays the man who endeavoured to possess and retain the mother's affection; thus in this sense we again come back to the father, and see that from the hero's point of view the distinction between the father and grandfather is not so radical as it at first sight appears. We perceive, therefore, that for two reasons the resolution of the original parent into a kind father and a tyrannical grandfather is not a very extensive one.

The foregoing considerations throw more light on the figure of Polonius in the present legend. In his attitude towards the relation between Ophelia and Hamlet are many of the traits that we have just mentioned to be characteristic of the Father-

daughter complex, though by the mechanism of rationalisation they are here skilfully cloaked under the guise of worldly-wise advice. Hamlet's resentment towards him is thus doubly conditioned, in that first Polonius, by the mechanism of "decomposition," personates a group of obnoxious elderly attributes, and secondly presents the equally objectionable attitude of the dog-in-the-manger father who grudges to others what he possesses, but cannot enjoy, himself. In this way, therefore, Polonius represents the repellant characteristics of both the father and the grandfather of mythology, and we are not surprised to find that, just as Perseus accidentally slew his grandfather Acrisios, who had locked up his daughter Danae so as to preserve her virginity, so does Hamlet "accidentally" slay Polonius, by a deed that resolves the situation as correctly from the dramatic as from the mythological point of view. With truth has this act been called the turning point of the play, for from then on the tragedy relentlessly proceeds to its culmination in the doom of the hero and his adversary.

The characteristics that constitute the Father-daughter complex are found in a similar one, the Brother-sister complex. This also may be seen in the present play, where the attitude of Laertes towards his sister Ophelia is quite indistinguishable from that of their father Polonius. Further, Hamlet not only keenly resents Laertes' open expression of his devoted affection for Ophelia—in the grave scene,— but at the end of the play kills him, as he had previously killed Polonius, in an accurate consummation of the mythological motive. That the Brother-sister complex was operative in the formation of the Hamlet legend is also evidenced by the incest between Claudius and the Queen, for from a religious point of view the two stood to each other in exactly the same relationship as do brother and sister. This conclusion may further be supported by the following—avowedly more tentative—considerations. The preceding remark about the two main traits in Polonius, those characteristic of a pompous father of a son and a grudging father of a daughter, gives room for the supposition that his family was in a sense a rough duplicate of the main family in the legend. This notion of duplication of the principal characters will be mentioned in more detail in the next paragraph, and the present line of thought will then perhaps become clearer. In the sense here taken Laertes would therefore represent a brother of Hamlet, and Ophelia a sister. This being so, we would seem to trace a still deeper ground for the original motives of both Hamlet's misogynous turning from Ophelia, and his jealous resentment of Laertes. As, however, this theme of the relation between siblings is of only secondary interest in the Hamlet legend, discussion of it will be reserved

for other legends in which it is more prominent (*e. g.*, those of Cyrus, Karna, etc.).

The *third* factor to be considered is the process technically known to mythologists as "doubling" of the principal characters. The chief motive for its occurrence seems to be the desire to exalt the importance of these, and especially to glorify the hero, by decoratively filling in the stage with lay figures of colourless copies whose neutral movements contrast with the vivid activities of the principals. This factor is sometimes hard to distinguish from the first one, for a given multiplication of figures may subserve at the same time the function of decomposition and that of doubling. In general it may be said that the former function is more often fulfilled by the creation of a new person who is a relative of the principal characters, the latter by the creation of a person who is not a relative; this rule however has many exceptions. In the present legend Claudius seems to subserve both functions, and it is interesting to note that in many legends it is not the father's figure who is doubled by the creation of a brother, but the grandfather's. This is so in some versions of the Perseus legend, and, as was mentioned above, in those of Romulus and Amphion; in all three of these the creation of the king's brother, as in the Hamlet legend, subserves the functions of both decomposition and doubling. Good instances of the simple doubling processes are seen in the case of the maid of Pharaoh's daughter in the Moses legend, or of many of the figures in the Cyrus one.¹ Perhaps the purest examples of doubling in the present play are the colourless copies of Hamlet presented by the figures of Horatio, Marcellus and Bernardo. Laertes and the younger Fortinbras, on the other hand, are examples of both doubling and decomposition of the main figure. The figure of Laertes is more complex than that of Fortinbras in that it is composed of three components instead of two; he evinces, namely, the influence of the Brother-sister complex in a way that contrasts with the "repressed" form in which this is manifested in the central figures of the play. Hamlet's jealousy of Laertes' interference in connection with Ophelia is further to be compared with his resentment of the meddling of Guildenstern and Rosencrantz. These are therefore only copies of the Brother of mythology, and, like him, are killed by the hero; in them is further to be detected a play on the "Twin" motive so often found in mythology, but which need not be further developed here. Both Laertes and Fortinbras represent one "decomposed" aspect of the hero, namely that concerned with revenge for a murdered or injured father.

¹ This is very clearly pointed out by Rank, *Op. cit.*, S. 84, 85.

It is instructive to note that neither of them shew any sign of inhibition in the performance of this task, and that with neither is any reference made to his mother. In Hamlet, on the other hand, in whom "repressed" love for the mother is even more powerful than "repressed" hostility towards the father, inhibition appears; this is because the stronger complex is stimulated by the fact that the object of revenge owes his guilt to the desire to win the mother.

The important subject of the actual mode of origin of myths and legends, and the relation of them to infantile fantasies, will not here be considered,¹ as our interest in the topic is secondary to the main one of the play of Hamlet as given to us by Shakspeare. Enough perhaps has been said of the comparative mythology of the Hamlet legend to shew that in it are to be found ample indications of the working of all forms of incestuous fantasy. We may summarise the foregoing considerations of this part of the subject by saying that the main theme of this story is a highly elaborated and distorted account of a boy's love for his mother and consequent jealousy of and hostility towards his father; the allied one in which the sister and brother respectively play the same part as the mother and father in the main theme is also told, though with secondary interest.

Last of all in this connection may be mentioned on account of its general psychological interest a matter which has provoked much discussion, namely Hamlet's so-called "simulation of madness."² The traits in Hamlet's behaviour thus designated are brought to expression by Shakspeare in such a refined and subtle way as to be not very transpicuous unless one studies them in the original saga. In the play Hamlet's feigning mainly takes the form of fine irony, and serves the purpose of enabling him to express contempt and hostility in an indirect and disguised form (*indirekte Darstellung*). His conversations with Polonius beautifully illustrate this mechanism. The irony in the play is a transmutation of the still more concealed mode of expression adopted in the saga, where the hero's audience commonly fails to apprehend his meaning. Of this, Saxo Grammaticus writes.³ "Falsitatis alienus haberi cupiens, ita astutiam veriloquio permiscebat, ut nec dictis veritas deesset nec acuminis modus verorum indicio proderetur." Here Hamlet plainly adopts his curious behaviour in order to further his scheme of revenge, to which, as we shall presently note, he had

¹Those interested in this subject are referred to the writings of Freud, Abraham, Rank and Riklin.

²My attention was kindly called to this point by a personal communication from Professor Freud.

³Quoted after Loening: *Op. cit.*, S. 249.

in the saga whole-heartedly devoted himself. The actual mode of operation of his simulation here is very instructive to observe, for it gives us the clue to a deeper psychological interpretation of the process. His conduct in this respect has three characteristics, first the obscure and disguised manner of speech just referred to, secondly a demeanour of indolent inertia and purposelessness, and thirdly conduct of childish and at times almost imbecillic foolishness (*Dummstellen*); the third of these is well exemplified by the way he rides into the palace seated backwards on a donkey. His motive in so acting was, by playing the part of a harmless fool, to deceive the king and court as to his projects of revenge, and unobserved to get to know their plans and intentions; in this he admirably succeeded. It has been maintained that even in the play this motive of spying on the king and disarming his suspicions was at work, but even if this was the case, and there are grave reasons for doubting it,¹ it is certainly more evident in the saga. Now, in observing the kind of foolishness simulated by Hamlet in the saga, we cannot help being impressed by the *childish* characteristics it throughout manifests, and Freud points out how accurately it resembles a certain type of demeanour adopted at times by some children. The motive with these children is further a like one, namely to simulate innocence and an exaggerated childishness, even foolishness, in order to delude their elders into regarding them as being "too young to understand" and even into altogether ignoring their presence. The reason for the artifice with such children most frequently is that by this means they may view and overhear various private things which they are not supposed to. It need hardly be said that the curiosity thus indulged in is in most cases concerned with matters of a directly sexual nature; even marital embraces are in this way investigated by quite young children far more frequently than is generally supposed. The subject is one that would bear much exposition, but it would be too far from the main theme of this essay to render justifiable its inclusion here.

It is highly instructive now to note the respects in which Shakspeare's plot deviates from that of the original saga; we are, of course, not here concerned with the poetic and literary representation, which not merely revived an old story, but created an entirely new work of genius. The changes are mainly two² in number. The first is as follows: in the saga Claudius

¹ See on the point Loening. *Loc. cit.*, and S. 387.

² Lesser points, important as they are, cannot here be followed out. Such is for instance the way Shakspeare accepts Belleforest's alteration of the original saga in making the Queen commit incest during the life of her first husband. The significance of this will be obvious to those who have followed the argument above presented.

(or Fengo, as he is here called) had murdered his brother in public, so that the deed was generally known, and further had with lies and false witnesses sought to justify it in that he pretended it was done to save the Queen from the threats of her husband.¹ This view he successfully imposed on the nation so that, as Belleforest² has it, "son peché trouva excuse à l'endroit du peuple et fut réputé comme justice envers la noblesse —et qu'au reste, en lieu de le poursuyvre comme parricide³ et incestueux, chacun des courtisans luy applaudissoit et le flattoit en sa fortune prospere." When Shakspeare altered this to a secret murder known only to Hamlet it would seem as though it was done, consciously or unconsciously, to minimise the external difficulties of Hamlet's task, for it is obviously harder to rouse a nation to condemn a crime that has been openly explained and universally forgiven than one which has been guiltily concealed. If Shakspeare had retained the original plot in this respect there would have been more excuse for the Klein-Werder hypothesis, though it is to be observed that even in the saga Hamlet unhesitatingly executed his task, herculean as it was. Shakspeare's rendering makes still more conspicuous Hamlet's recalcitrancy, in that it disposes of the only justifiable plea for non-action.

The second and all-important respect in which Shakspeare changed the story, and thus revolutionised the tragedy, is the vacillation and hesitancy he introduced into Hamlet's attitude towards his task, with the consequent paralysis of his action. In all the previous versions Hamlet was throughout a man of rapid decision and action, not—as with Shakspeare's version—in everything except in the task of vengeance. He had, as Shakspeare's Hamlet felt he should have, swept to his revenge unimpeded by any doubts or scruples, and had never flinched from the straightforward path of duty. With him duty and natural inclination went hand in hand; from his heart he wanted to do that which he believed he ought to do, and was thus harmoniously impelled by both the summons of his conscience and the cry of his blood. There was none of the deep-reaching conflict that was so disastrous to Shakspeare's Hamlet. It

¹Those who are acquainted with Freud's work will have no difficulty in discerning the sadistic origin of this pretext. (See *Sammlung kleiner Schriften*, Zweite Folge, 1909, S. 169.) The interpretation of an overheard coitus as an act of violence offered to the mother is frequently an aggravating cause of hostility towards the father.

²Quoted after Loening, *Op. cit.*, S. 248.

³This should of course be fratricide, though the word parricide was occasionally used in old French to denote a murder of any elder relative. It is conceivable that the mistake is a "*Verschreiben*," unconsciously motivated in Freud's sense. (See *Psychopathologie des Alltagslebens*, 1907, Cap. VI.)

is as though Shakspeare had read the previous story and realised that had *he* been placed in a similar situation he would not have found the path of action so obvious as was supposed, but on the contrary would have been torn in a conflict which was all the more intense for the fact that he could not explain its nature. In this transformation Shakspeare exactly reversed the plot of the tragedy, for, whereas in the saga this consisted in the overcoming of external difficulties and dangers by a single-hearted hero, in the play these are removed and the plot lies in the fateful unrolling of the consequences that result from an internal conflict in the hero's soul. From the struggles of the hero issue dangers which at first did not exist, but which, as the effect of his untoward essays, loom increasingly portentous until at the end they close and involve him in final destruction. More than this, every action he so reluctantly engages in for the fulfilment of his obvious task seems half-wittingly to be disposed in such a way as to provoke destiny, in that, by arousing the suspicion and hostility of his enemy, it defeats its own object and helps to encompass his own ruin. The conflict in his soul is to him insoluble, and the only steps he can make are those that inexorably draw him nearer and nearer to his doom. In him, as in every victim of a powerful unconscious conflict, the Will to Death is fundamentally stronger than the Will to Life, and his struggle is at heart one long despairing fight against suicide, the least intolerable solution of the problem. Being unable to free himself from the ascendancy of his past he is necessarily impelled by Fate along the only path he can travel—to Death. In thus vividly exhibiting the desperate but unavailing struggle of a strong man against Fate, Shakspeare achieved the very essence of the Greek conception of tragedy.

There is therefore reason to believe that the new life which Shakspeare poured into the old tragedy was the outcome of inspirations that took their origin in the deepest and most hidden parts of his mind. He responded to the peculiar appeal of the story by projecting into it his profoundest thoughts in a way that has ever since wrung wonder from all who have heard or read the tragedy. It is only fitting that the greatest work of the world-poet should have been concerned with the deepest problem and the intensest conflict that has occupied the mind of man since the beginning of time, the revolt of youth and of the impulse to love against the restraints imposed by the jealous eld.

SPONTANEOUS CONSTRUCTIONS AND PRIMITIVE ACTIVITIES OF CHILDREN ANALOGOUS TO THOSE OF PRIMITIVE MAN¹

R. A. ACHER

Much has been said of late years about following the child's native interests and instincts in directing its education. But if we ask just what these interests and instincts are the answer is not so readily given. A different answer would very probably be given by different leaders in this field. Children grow up in an environment adjusted largely to adult life and adult ideals, and are still pretty firmly in the grip of the traditional school system. Therefore their spontaneous activities are still more or less obscured and neglected. But it is just here that most may be learned about children's instincts and interests. These are nowhere more fully revealed than in their spontaneous activities, plays and constructions. It is here that they get away from the influence of adult life and complex society at least partially, and build up their simple crude world with all its primitive aspects and attributes. They thus give expression to their mental and physical capacities and needs. These activities of children have not received the attention which they merit, although considerable valuable work of this kind has already been done. The pity is that what we do know in this field as a result of previous study is not more fully applied. The student of child study who is familiar with the material that has been collected in the past two decades on this subject cannot but be impressed with the great difference which exists between the theory of education which this child study investigation suggests on the one hand and the theory which under-

¹ Acknowledgments for returns are due to Miss Lillie A. Williams, New Jersey State Normal School; Mr. W. S. Munroe, of the Westfield, Mass., Normal School; Miss Frances Judson, of the Chicago Kindergarten Institute, and Margaret Pritchard, of the Philadelphia Normal School for Girls.

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lies much of the actual school work on the other. It is quite evident that there are many instincts and interests of children already revealed by these investigations which are not taken account of and utilized by the school programme of to-day.

The following investigation of children's spontaneous constructions and primitive activities is made in the hope that something of value may be added to the large mass of data already collected on this subject and that a clearer, saner insight into the child's nature and needs may follow.

The minute details of the investigation are given so that the picture of the children's activities may be as complete as possible. No effort is made to interpret all these facts, a statement of the facts being thought worth while.

Some attention is also given to the analogy between the children's activities and constructions and those of primitive man with the view of throwing some light on the possible origin of these activities and constructions of children. It is very improbable that the child begins life with a clean slate, so to speak, and that imitation and experience account solely for his early activities. Even the idea of Groos that children are somehow preparing for their later life work seems hardly tenable, except in so far as it is incidentally the case. Investigators are coming to recognize a psychic evolution as well as a physical one. From this point of view the mental life of man cannot be adequately understood except by a consideration of its phylogenetic origin. The child's mental and physical life according to this view, is largely a budding forth of the early, vital, racial experiences. That is, in the evolution of the race its reactions towards its environment became a part of the vital inheritance of the generations following. The part which the unconscious psychic influence played in this evolution is now thought to be far greater than was ever heretofore thought to be the case. How far this is true and what part is played by imitation and the experience of the individual cannot of course be stated. But when children seize upon a certain line of activity with enthusiasm, and delight and persist in it for more or less extended periods of time, there is certainly more at the basis of this phenomenon than mere imitation. For frequently they give expression to unique and original ideas. Again, when these activities are very similar to those which the race very probably experienced in its early history, we are certainly justified in giving some consideration to their phylogenetic origin.

The significance of this view, if true, for purposes of education can hardly be overestimated. It thus becomes the imperative duty of educators to follow this course of development and work with the current of psychic evolution and not against it as is so often the case at present.

In order to compare children's activities more readily with those of the race this investigation was made along the lines of their spontaneous constructions and play with blocks, stones, sand and earth, and snow; their use of strings, points and edges, and their striking and throwing propensities were also considered. Their attitude towards clothing and their desire to modify their bodily form also received attention. The material was largely gathered from the returns of a questionnaire which was answered by the students of several normal schools. Thus the returns have the merit of coming from teachers or persons expecting to become teachers and actively engaged in preparation for their work. Those who had the questionnaire in charge in these schools were trained in collecting data by this means. This greatly increases the value of the material thus gathered.

BLOCKS

The reports, covering observations of 186 boys and 269 girls, show that the tendency on the part of children to use blocks for purposes of construction is very marked. It begins even before the third year and continues up through the seventh year. There is great satisfaction in this and hours are often spent in constructing a great variety of things. The material used indicates that if they have not one thing with which to build they will use any other which lends itself to their purposes. This is shown by the fact that when blocks were not available they used bricks, dominoes, spools, corn cobs, pine cones, sticks of wood, boards and blocks from the builder's discarded pile. The usual construction is a house, this being mentioned in the majority of cases. What does it take to satisfy the child's idea of a house? Very young children are satisfied with a solid mass of blocks with no definite form. Somewhat later blocks are placed in two rows with a space between and another row of blocks on top for a roof; or a board may be used for a roof. Many varieties of this are built due to the difference in age and ingenuity of the children. Some omit blocks in the building for doors and windows, and put steps before the door, lay walks leading from the house and may even put a block before the opening supposed to be the door in such a way as to show the door ajar. Others make simply rectangular hollow forms with no provision for either doors or windows. Again others build two or even three stories, with pasteboard or something of the kind for floors, and partitions of blocks between the rooms. These various rooms are often appropriately furnished blocks representing the various articles of furniture. Many children centre upon some one part of a house or other building as for example a fireplace, chimney, stair steps, post-office boxes, pew and altar or church steeple, etc., and omit all

else without any feeling of incompleteness whatever. On the other hand not a few children of similar age go far towards completing the whole surroundings; not only representing all parts of the house in a rude way, including bay windows, piazzas, and perhaps a tower, but even completing the yard with fence around it and gate leading to the road. This gate is frequently rather elaborate with two blocks for posts and another on top of these; or else an arch is built over the gateway. Some extend this so as to represent a whole farm, or even a rural community, or village. The farm includes usually a barn with stable and horses in it, henhouse, pigpen, fences, fields, with cows in some of them. The village consists of store, postoffice, church, shops and streets.

Several interesting things are revealed by this building. In the first place the vagueness of children's ideas is beautifully shown here. They have the crudest notions of even the most familiar objects. Of course some allowance must be made for their inability to execute and embody their ideas in objective form. But their incomplete notion of objects is shown by the fact that they often emphasize one or the other part of an object to the exclusion of other perhaps more important parts, and still consider it complete; for example, the chimney, stair steps, or steeple is called a house or church; a well may be built on top of the ground with blocks and offer no difficulty whatever.

Children rarely undertake to produce a copy of their own house or other buildings with which they are familiar. Only a very few cases are reported in which this was attempted. The children seem to have vague conceptions of their own, the expression of which is more satisfying to them than the making of a copy of one they are acquainted with. The house which they build is sometimes called a mansion, or castle, of which they have heard some story or fairy tale. It may be one which they are going to have when they grow up. It thus seems that the child can more readily represent a type of any familiar object than he can produce a copy of it.

Other forms of construction are towers, pyramids, columns, and steeples. These were mentioned in about 75 per cent. of the reports. The chief aim is to build them as high as possible, the higher the more delighted is the builder. The blocks are either all used in this building or else piled up until the tower falls. The falling of the tower is enjoyed very much and often it is pushed over with great pleasure. These towers are built in various ways; sometimes two blocks are set upright and a third placed across the top, and others placed similarly on top of these. Others pile them in rectangular columns with square bases or in the form of a pyramid. The steeple of the church

is usually the principal part of the building, and is built high as possible. It is quite evident from the returns that children derive especial satisfaction from placing the blocks one upon the other. This seems to be one of the first uses which they learn to make of them, and is continued through the block-building period. Another interesting exercise is to place dominoes or other similar blocks in such position that when one is pushed over it pushes over the one next to it and so on until all are down. These rows are often made as crooked as possible to still accomplish the end desired. The child seems to learn for the first time that it has the power to arrange blocks so that the result will be greater and farther reaching than is ordinarily the case. It learns to extend its power of doing things in a new and, to it, surprising way. This is also done in piling up the blocks and having them fall.

Other things which are built are walls enclosing space which may be called a sheepfold for protection against wolves. Or they may be protection against enemies of the children, or prisons. They are varied in shape, often circular or some other odd form. Children also build engines, cars, bridges, smokestack, blacksmith shop, boat, Noah's ark, fountain, mountain, fire engine, park, etc.

The important fact here is not their success or failure in attempting any of these things; but rather that they would undertake to represent such varied and often complex things. The interest which children manifest in this construction play could certainly be turned to great advantage in their physical and mental development, if properly utilized. Both the home and the kindergarten and even the lower grades could do much more than is done in providing for and encouraging this interest. The possibilities here have not yet been fully developed. If the blocks were made somewhat larger than is ordinarily the case and of heavier material, the child would get excellent exercise in making these constructions. The effort of the child to put into objective form his ideas is certainly stimulating in the highest degree, and its encouragement would do more for the child than much of the more abstract work that is being done in the schools of to-day. The study by the teacher of these crude efforts of the child would be a very profitable thing. In no other possible way can one get a better notion of the child's limitations and difficulties than by a sympathetic study of these early constructions.

SAND AND EARTH

Just what do children construct with sand and earth and how do they play with it? The returns fully agree with common observation in that they show that children like very much to

play with sand and earth. Here also a great variety of constructions are attempted, and as children continue their interest in this sort of construction to a much later period than with blocks they produce much more elaborate and complex structures. Perhaps the most favored effort of the children is that of well-digging. Both boys and girls enjoy it. Here, as elsewhere, the age and natural aptitude of the children determine the nature of the product. The younger children merely dig holes in the ground and fill them with water. When a little older they often dig a deep, wide hole wall it up with boards or even with stones and make it have more the appearance of a real well. Some of the older children even struck water and made a real well. Two boys 10 and 11 dug a well 6 feet deep and walled it up.

Some children like to dig holes into the ground without any definite purpose. These are of various sizes and depth and are often called a lake or pit. A favorite effort is to dig tunnels between two holes. The digging of underground passages seems to have a peculiar fascination for children. Canals and ditches to conduct water are also interesting to them.

The greatest delight, especially for the boys, is digging caves. There seems to be a peculiar interest in digging a hole big enough to contain three or four boys, and then covering it up and concealing it so that no one can find it but the ones who dug it. Here much time is spent in hiding from people, eating fruit and other food pilfered from the pantry, etc. Girls rarely participate in this and when they do they assist their brothers and do not take the initiative. The ages of both boys and girls range from 7 to 15 years. The greater number are from 11 to 15. These caves vary in complexity from mere holes in the bank or ground to large sheltered rooms connecting with each other by passageways. Several illustrations may make clear their plans.

"Three boys, 10 to 11 years, dug cave in hillside. Covered floor with pine needles, hung burlap across doorway and lighted it with tallow candles. Here potatoes were roasted and eaten, and hours spent every day." "M— 14, and others, dug cave in river bank, had stove and furniture in it; also fishing tackle, boat and oars. Kept it in good shape. Stole watermelons, apples and chickens of neighborhood and ate them." The work that is often done in constructing these caves is almost incredible for boys of this age. Just recently the writer has had an excellent opportunity to observe a case of digging and building such a cave house. A boy of 12 and his 10-year-old sister conceived the idea of digging out the central portion of a mound about six or seven feet high and eighteen or twenty feet in diameter, and then constructing a house of this by placing

board walls on the sides and erecting a roof over the top. They also made arrangements for a chimney and for at least two rooms, and fully expected to sleep in it at night. Although the boy is regarded even by his father as having an aversion to work of any kind, he went to work with such enthusiasm that the digging was completed in a few days. Several tons of earth were removed and part of it wheeled some distance from where it was dug. His sister also worked at it, and he pressed as many of his companions into the service as he could but, nevertheless, the bulk of the work was done by the boy. He persisted from day to day until the digging was completed. His plans are elaborate and the problems which he will have to solve before completing it will be many and complex.

It is very evident that the interest in these semi-cave-houses leads boys to do a great amount of labor, planning and devising. The educational value of this is by no means unimportant. It brings them face to face with practical difficulties and problems and gives them a chance to use their inventive genius. From this point of view what might ordinarily seem to be sheer nonsense and a waste of time and energy becomes a means of development that can hardly be equalled in any other way. These activities should therefore receive the most careful consideration by adults. Here, as elsewhere, the great gap between youth and maturity must constantly be kept in mind if the right attitude is to be maintained towards these activities of children and youth.

The house-building propensities of children with blocks and then a little later by digging into the earth, and also into the snow, as will be seen later in this paper, indicates pretty clearly a deep-seated basis for these constructions. Imitation seems hardly sufficient to account for them; for many of the structures are widely different from any which the children have seen. It might be argued that since children are immature both physically and mentally we should expect them to build just such structures as they construct and as savages might build. But this fails to account for the universal and intense interest in this form of activity. We are almost forced to conclude that it is the expression of the psychic tendency formed not only in early man but in many of the higher animals to build some sort of habitation.

Another prominent interest of children is shown in their desire to bury things. Nearly all the children mentioned in the returns were at some time much interested in this. Among the objects buried were broken dolls, birds, chickens, cats, dogs, mice, grasshoppers, marbles, knives, books. In most of these cases the burial is for the purpose of disposing of the dead body of the animal. The burials are generally conducted

in groups and with more or less ceremony. Flowers are in evidence and the mourners make their demonstrations with some real feeling. The body is wrapped in some kind of cloth, placed in a small box and then lowered into the grave and covered. For days and even weeks afterwards the grave is visited and sometimes flowers placed upon it. Sometimes the grave is reopened and an inspection is made of the body. Children here become acquainted with the mystery of death in a way that may prepare them for the ordeal of burying some one of those nearest them later.

Some children seem to enjoy burying anything, such as knives, forks, spoons, pins, thimbles, spools. Several from 3 to 5 years of age buried playthings or pennies, expecting them to grow. Others buried marbles, toys, books, to keep them. One girl of 4, planted a ring expecting it to grow into a large ring.

Children also like to partially bury themselves or others in the sand. Usually the whole body is covered except the head, but sometimes only the hands and feet are buried.

The other objects made with sand and mud are many and varied. Chief among these are mud pies, cakes, houses, mounds, forts, walks, bridges, dams, animals, people, dishes, farms, villages. The making of mud pies and cakes seems to be limited largely to the girls. The returns show that 158 girls made these but only 4 boys. Houses are built by both boys and girls. They consist of various degrees of complexity from mere heaps of sand with small holes for windows and doors to houses with several rooms, yard with fence around it, flower beds, paths, driveways, barn, pigpen, well, trees or small twigs, even communities and villages are attempted. These are of course miniature in size. Some make an Eskimo house by burying one hand under a heap of sand, packing it tightly and then gently removing the hand.

Mounds are made by piling the sand several feet high. Sometimes they are tunneled through. Forts and walls of various sizes are also built. Animal and human forms are attempted in sand and mud. These are of all degrees of perfection from the rudest representation up to full figures with arms, legs, features. Since these same things are done by children with snow, a more complete discussion will be given under that subject.

STONES

The data on stones is treated under the following heads: Things made of stones; collections of stones; charms and amulets; breaking and chipping stones, pounding stones together and using them as hammers; stones set on edge or

superposed. Each of these will be discussed in the order given.

It is evident that stones enter largely into the play of children. In nearly all the returns, including 281 girls and 101 boys from 5 to 9 years of age, were found statements such as these, "I liked to play with stones in my childhood." "I played much with stones." There were reported more than thirty different uses made of stones. Their use in making playhouses was most prominent, it being mentioned in one hundred and twenty-two reports. The houses are very simply represented and usually consist of only one or two rooms; but in a few cases four or five rooms were mentioned. The walls are merely rows of stones on the ground with blank spaces for doors and windows, and single rows of stones to represent the rooms. In only four per cent. of the cases were the stones superposed and an effort made to enclose the rooms on all sides and construct a roof. Thus we see again that the child's conception of a house is not very clear and is easily satisfied. This will be illustrated again in the other parts of this paper. A few quotations will show what was done:

F., at 7. "Made outline of house with stones, leaving space for door and windows, and left stone to knock on when any one wanted to come in."

F., at 6. "Marked off the extent of my playhouse with stones placed end to end."

F., at 8. "Marked off playhouse with white stones, also rooms."

The houses were rarely large enough for the children to enter them. The merest hint of walls and rooms was entirely satisfactory for a complete house. Two sides and one end often sufficed for a house. In fifteen cases mention is made of dividing lines or partition walls within the houses to separate rooms. One girl reports that she built a playhouse with walls four layers wide and two feet high and representing a real house in number of rooms. Four houses were built like caves and were called cave houses. One boy said, "I made a cave house out of stones and some boys claimed it as theirs and I had to drive them out." Whenever a house was large enough they would always crawl into it, as is shown by the following typical case: "Built playhouse which was simply stone walls with cornstalks for roof into which we often crawled."

The houses were supplied with furniture and kitchen utensils in 44 cases. Smooth, flat stones were used as tables and chairs for tea parties. Various shaped stones and granite chips were used for dishes. Beds for dolls were made of stones in one corner of the room. Ovens and furnaces were mentioned several times. They were built by placing flat stones on bot-

tom, and placing others on the sides and then placing a flat stone on top.

The next most frequent use that was made of stones was playing store or some other form of interchange. Stones were used as articles of food or merchandise in 45 cases and as money in 42 cases. When used as articles of food they were marked in some way or else different shapes were the distinguishing marks. When used as money the distinction depended upon the size or color of the stones. Stones represented all kinds of groceries such as eggs, nutmegs, lump sugar, apples, potatoes, raisins on cake, candy, various kinds of cakes, etc. In 2 cases scales were mentioned in connection with store play. These consisted of boards balanced on a rock.

Bright, sparkling stones were used as decorations for the rooms. Some decorations were made by pressing stones into the sand so as to make pictures of animal forms or human faces. In 43 cases stones were used to form the border of flower beds, walks and roads. The stones so used were carefully selected and arranged with an effort to make a beautiful effect.

In thirty cases gardens were made in connection with the house by laying rows of stones around a plot of ground or setting them upright so as to make a fence or wall. 75 children used stones as a means of separating different tracts of land from each other. A frequent expression was "Marked off extent of homes and gardens in play so claims would not overlap and cause trouble." Only eight children constructed barns in addition to their houses and in no case was a barn built without a house. The dawning of child consciousness is nowhere more fully revealed than in these early constructions of children, and for this reason the details have been so minutely given.

The use of stones in damming up streams was mentioned in the play of 29 children. This was done either to make a lake or to make a waterfall or rapids; this seems to have been a habit with some immediately after a rain in the spring and early summer. The making of bridges was mentioned 9 times and the making of canals and dykes once. The bridges were formed by throwing stones into the water until they extended above its surface. One little girl built an arch across a small stream by setting stones on end and leaning them against each other and then piling others on top of them.

The graves of dead pets such as cats, chickens, birds, dogs, etc., were marked with stones by fifteen children. The following are typical statements: "Placed stone at grave of canary." "Got chips from the marble cutters and used them as grave stones for bird and cat." The ceremony accompanying these burials and the erection of the tombstones was more

or less elaborate and in a few cases the initials of the pet's name were scratched or written on the stone.

Sixteen references were made to the use of stones for marking or writing with or upon. The scratching of pictures on sandstone was very much enjoyed. An old piece of slate found in the creek bed was often carefully saved and used in writing in preference to a well sharpened pencil. Colored stones that mark were valued highly and placed in the collection of valuable stones. These stones were used to mark on fences, rocks, or any other place that would receive the marks which were usually the child's own name or initials. Several children wrote their initials on stones and threw them away. This strange desire to write their names in as many places as possible seems to persist through the adolescent years.

Other uses of stones by one or more children were as follows: Six mentioned the use of stones to represent families; father was large stone, mother smaller one and children still smaller ones. Six children played guessing or other games with stones. Some awarded stones as prizes for good conduct; others rolled stones off of a ledge or down a plain; put them in tin cans and rattled them in giving the Indian war dance; fastened a stone to a handle and made a tomahawk; buried stones; used them as jewels; as a throne for a king; as means of representing trees and other plants; arranged them in concentric circles, squares and triangles; used sharp stone to cut down a cherry tree; used stones to scatter along a path so as to tell the way to return; or one would scatter stones and another would try to track her by them; and one little girl placed a stone beside a tree and went to it from day to day to see how fast it would grow.

COLLECTIONS OF STONES

This properly comes under children's collective instinct and as this has already been treated by Mrs. C. F. Burk, it will merely be touched upon here. Mrs. Burk has shown that among the things collected by the 1,200 and more children upon whom she reported, stones were collected by 18% of the boys and 15% of the girls. In the returns which the writer has examined over 50% of the children made stone collections; 175 girls and 46 boys. The chief points of attraction were color, shape, smoothness, brilliancy and beauty. Under shape the attributes mentioned most were smooth, flat, round. Under color, white, pink bright, brilliant, red, dark blue, yellow, sparkling, stained; those with holes in them were also attractive. 144 children collected stones on the account of color, 48 because of odd shapes, 38 because they were pretty, 24 because they were smooth, 10 because they were flat and 12 because

they were round. A few quotations follow: F., at 6. "Collected all the pretty stones I could find, liked round ones." F., at 7. "Picked bright stones and hid them for my own." M.,—"Collected stones and arranged them in concentric circles." M., 10. "Collected Indian arrow heads and other strange stones." M., 8. "Collected stones for beauty or with crystals in them." F., 8. "Collected stones of peculiar shape and color to decorate flower beds." F., 6. "Collected stones which would make marks, especially those which would make red or blue lines." F. "Collected stones after rain; liked smooth, round, black, and white ones." M., at 9. "Had great liking for little stones and pebbles of peculiar shape and color, kept them in pocket or hid them; finally I lost interest in them and threw them away." F. "Collected those with holes in them, put on string and wore as bracelet."

One teacher tells of taking 25 children to the seashore. All, at once, began to collect stones of various colors and shapes. The boys put them in their pockets until they bulged out. In nearly all cases the stones appealed almost exclusively to the sense of sight of the children, as is shown by the expressions used in characterizing the stones selected; such as strange, curious, odd shaped, smooth, flat, bright, pretty, sparkling, brilliant, glittering red, yellow, pink, white, and stained. In a few instances the appeal to the sense of touch was mentioned; as for instance M., "I like to carry a stone in my pocket because I like the feeling of it." Again the appeal to the æsthetic in the child was prominent. The use of the stones as decorations of the playhouses, gardens and flower beds was frequently mentioned; also their use in personal adornment, as in making bracelets. They were also placed away on shelves or kept in boxes where they would be looked at and admired frequently. All this shows the groping of the child's undeveloped instinct for the beautiful.

The use of stones for charms or for luck was also a motive in the collection of stones. 59 girls and 5 boys collected stones with special charms. The supposed influence of the stone would keep them from harm, help gain some desired end, or insure some unlooked for good luck. The lucky stone had some distinguishing mark of color, form or size. In about 20% of the cases the stones were described as either white, or smooth white. Other marks were a white ring or a black ring around the stone, black stone with band of gray about it, stone with letter on it, stone with hole in it, smooth stone, stone with brownish color, or red stone. The specific purpose of carrying or possessing these stones will be better understood by giving a few of the more common quotations: F., at 8. "Wore stone with hole in it around my neck for good luck." F.

"Carried red pebble in pocket for months, believing it would bring good luck." F. "Yellow stones were used as charms or as lucky stones, if we lost one the day we found it bad luck followed." F. "Stone with white ring about it had charm; I buried it, said luck three times, and made a wish." F. "Little white stones had charm to keep off warts from my hands." F., 8. "Smooth brown stone is lucky stone; carried it a certain time and then threw it over left shoulder, not seeing where it went."

These illustrations show how strikingly similar the child's acts are to those of man everywhere in the early stages of civilization. Mrs. Burk has shown that the instinct for collection has a rise, growth and decline. Ellis thinks the collecting instinct a survival of fetichism, which in turn is a form of savage worship. Children are imaginative and have little experience to base their theory of things upon. They see phenomena every day which they can't account for. They feel the wind blow, but they know not "Whence it cometh or whither it goeth." Their curiosity is aroused a thousand times and left unsatisfied. They see forces outside of themselves which they cannot understand. Animism, therefore, is as easy for them as for savage man. Thus their minds afford a rich soil for suggestions of all kinds touching charms, lucky stones, amulets and fetiches concerning which the very atmosphere even to-day is still overcharged. This very suggestiveness and ready imitation may signify a racial instinct and have its roots way back in man's primitive life.

The use of stones for hammers was mentioned by 47 individuals, 25 girls and 22 boys. The chief uses to which these hammers were applied were to crack nuts, to crush things with, to make sparks fly by hammering stones together, to pound things into the ground, and to make noise by knocking stones together in order to call playmates.

Stones were broken, chipped or hammered into powder by 87 children. They liked to crumble soft stones to make powder for mud or medicine or to mix with water and make paint. Stones were chipped to see what was inside, especially to see where crystal, mica, quartz or color came from; or were broken for decorations. Again others said they liked to break stones but they could n't tell why. Some of the more common expressions given were: M. "Pounded up soft stone to make mud." M., 8. "Sandstone was ground up for medicine or sugar; also broke stone with mica in it to get it out." F. "Gathered different colored stones, pounded them into powder and put into bottle in layers." F. "Liked to break and chip stones and pound them into powder, mix with water and mould into little balls and dry in the sun." F., 7. "Liked to crack stones to see

what was in them." F., 8. "Broke stones and chipped them, liked the smell, it was a powdery smell." F., 6. "Broke prettiest with hatchet to see where color came from, also tried to break flint to see where fire came from." F. "I liked to strike stones to see sparks fly." 18 children set stones on edge. The following quotations will show why. M., 2. "Stands rocks on edge and screams with delight." M., 7. "Stands stones on edge for wall." M., 5. "Stands stones on edge and knocks them down as enemies." These are typical.

Stones have played a large part in the story of human evolution. Perhaps more than will ever actually be understood and appreciated; and indeed more than had been thought to be the case until archæologists in recent years began to dig up, classify and study the products of the first steps of civilization. These investigations are revealing a richness of material that bids fair to unlock many of the mysteries of man's early advance in his conquest of, and triumph over, his environment. They show that stones of various kinds were the one great means by which man succeeded in making an advance over his four-footed brethren. Rude implements and tools of all kinds show that the first evidences we have of past history reveal a state of civilization already pretty complex. The ages that elapsed before man was able to modify stones and adapt them to his use, as shown by archæological excavations, must have been of great length. Of course these must always be a closed book except in so far as their nature is revealed by various tribes of to-day occupying the same stage of civilization. Yet this age, of which there is absolutely no vestige of record remaining, was the real beginning of man's upward movement. Here man began as the only tool using animal and this was what helped him in his contest for supremacy with man and beast. These earliest tools were stones. Perhaps he first found a sharp stone and used it and ultimately tried to improve it; and so came the great number of primitive tools such as chisels, arrow heads, scrapers, saws, hammers, etc. Thus was begun the stone age in the dark and distant past. Stones were the weapons of offense and defence; the fierce struggles that were waged against man and beast were won by the use of superior implements of warfare made of stone. Those who could make the sharpest instruments and could use them most skillfully survived. In short their whole life was centred around stones, and progress was made possible by their use.

In historic times stones played a large part especially in the early life of the people of Europe and Asia. Among the Hebrews the stones were regarded as instruments of death. Precious stones were often mentioned as symbols of divine glory. The ceremonial use of stone and flint has been prominent in

all ages. As an appeal to the sense of beauty stones have had a great effect throughout history and even to-day man's most precious ornament is a stone. Since stones have played such an important part in man's evolution it is not strange that they should make a strong appeal to children and enter so prominently into their play. It is another illustration of the psychic adaptation of the race to its environment in its evolutionary struggle. It seems but wise to use and direct this interest and give it full and free play. The child may thus become acquainted with the facts that will later help him interpret the composition of the earth's surface; but what is better he will be stimulated through this activity to healthful, physical and mental life. In building with them he will need to plan and think and then execute. Even if the stones are rough, and he bruises his hands a little now and then, this will not be amiss in his contact with the larger life that awaits him. Similarly all other activities of children in connection with stones mentioned above are healthful and might well be encouraged.

SNOW

Snow has ever been the delight of childhood. The dreariness of the late autumn months is made more endurable because of its prophecy of the cover of whiteness that will soon spread over the landscape and hide the dreariness from view. And then a thrill of delight fills the youthful heart as the first flakes begin to fall, "And driving o'er the fields seem nowhere to alight." No more study and work now until the pent up enthusiasm is spent in happy glee and play out in the midst of the "Swarm and whirl dance of the blinding storm as zigzag wavering to and fro crossed and recrossed the winged snow." Then the wading through the drifts, the digging through the "solid whiteness;" and finally when the warm south winds change the dry, light snow into the moist, plastic moulding material, the snowballing, the making of human forms and the playing of games—all add to make snow truly a joy of childhood. Especially on days when snow is falling are children stimulated to a higher pitch of activity and enthusiasm. They become more spontaneous and unrestrained in their play. There is new vigor and life pulsating throughout their bodies and they abandon themselves to the impulses of the moment more than at other times. This is shown by the following quotations:

F., 20. "I was always happy when I saw it begin snowing and when I could run and play in the snow."

F., 19. "We children used to love to jump into the deep snow from a wall. We would bury each other in the snow with laughter and shouting."

F., 22. "I liked to play in snow. I helped my brother dig

caves in big drifts, packed snow together for forts and made hills, working for hours for one or two slides down it later."

F., 20. "Whenever it snowed we built a fort and had a snow fight. I remember doing this from the time I was six. We made men and animals and played circus."

F., 20. "Threw snow by the handful into people's faces and tried to wash them. Made snow men and then run and jumped on top of them and tumbled them down."

This overflow of life is probably due to the brightness of the snow. In the article on "Reactions to Light and Darkness," by Drs. Hall and Smith, it has been found that there is a close connection between the general feeling tone and the amount of direct sunlight. The bright effect of the white snow no doubt has a tendency similar to direct sunlight.

The returns covered 411 observations of children, and reminiscences. Of this total number 159 were boys and 252 were girls. These children were somewhat older than those playing with blocks, ranging from 6 to 12 years of age, and worked more in groups than in playing with blocks. There was a marked tendency to make constructions and mould forms of various kinds. Under certain conditions snow is especially adapted to this use. It packs easily under pressure, and yet it can be changed in form with little effort. With the child's instinct for constructing things it is not strange, therefore, that snow should be made use of for this purpose.

Nowhere, perhaps, do we have a better opportunity to study the child's crude imagination and primitive constructive power than in the snow constructions. This is especially true with regard to his efforts to mould the human form into the so-called snow man that is so frequently made. Of the total number of children reported 319, or more than 75%, attempted the construction of snow men. Like primitive man, the child is easily satisfied with his product, be it ever so rude and imperfect. The snow men were more or less alike. They usually consisted of three balls of varying dimensions piled one above the other and shaped where they joined. The lowest ball was always much the largest with the smallest on top forming the head. The height of the whole was determined by the age and height of the children. The tendency was to make the figure as high as they could reach. The arms were frequently omitted, and not very successfully done when attempted, and the fingers were not indicated. The parts of the body that received most attention were the face and the upper part of the trunk; but nearly all the effort was centred upon the face, as will be shown by the following typical quotation: M. and others. "Made snow man by rolling three balls. Punched holes for eyes, nose and mouth."

M. and F. "Made snow man; used bits of coal for eyes, stones for nose and mouth, and broom for musket."

F., 5. "Made snow man by rolling snow balls and placing one on top of the other; used coal for eyes, nose and mouth and row of buttons down in front."

M. and F. "Made snow man; used stones for eyes, used stick for nose, drew ears and mouth with pointed stick, placed clay pipe in his mouth and hat on his head."

M. and F. "Made a snow man by rolling long balls for arms and legs, used coal for eyes and small ball of snow for nose, also for ears, made long line for mouth, put a stick in it for a pipe, laid stick on shoulder for gun."

M. and F., 10. "Made snow man with marbles for eyes, hay for whiskers, excelsior for hair, and a piece of red flannel cloth for a mouth." The eyes received more attention than any of the other features. While all of the children did not go into details concerning their snow men, 52 mentioned the eyes especially and made great effort to represent them with effect. The usual representations of the eyes were stones, marbles, pieces of coal and short sticks. The mouth seems to have been second in the attention it received; it was mentioned 50 times and was usually made of sticks or just a mark in the snow or a row of cranberries or red cloth. The nose was mentioned less frequently and the ears a very few times only. It may be due to the fact that the ears are less easily represented. Very few attempts were made to represent the legs of the snow men and in all cases these were made by the older children from 10 years up. The younger children were satisfied with a large ball as a foundation upon which to construct the remainder of the form. It would thus seem that they centred their attention upon a few of the more important portions of the human form to the neglect of the others. Only three attempts were made to make snow women. One of these, a girl of 7, rolled large balls of snow and made a woman by shaping the bottom ball like a skirt and made her with her arms crossed, which must have embodied her idea of woman's characteristic position. It is not clear why they gave so little attention to the making of feminine forms.

Perhaps it was due to the fact that the boys took the initiative in the constructions. There is some evidence of this as the girls frequently stated that they helped their brothers. But the more probable reason is that man's more active, out of door life appeals more strongly to the children's imaginations.

The attention to the dress of the forms made reveals the crude conceptions which children have concerning the representation of clothing. Thirty-six children represented the

coat with a row of pieces of coal down the front for buttons. Six placed a hat on the head with no other effort to represent clothing while the rest made no effort whatever to indicate clothing. There was considerable effort to put content into the forms and express ideas with them. Father and son were represented by tall and short forms. Some children made whole families—the father, mother and children; put a pipe in the father's mouth to distinguish him, and a broom into the mother's hand to distinguish her while the children were made smaller and of different sizes. Soldiers were indicated by placing long sticks on their shoulders for muskets. They were usually stationed near a fort to guard it. Various characters such as old King Cole, Santa Claus, and others were given some distinguishing mark.

The tendency to make caricatures was noticeable. Some were dressed up like scarecrows, others had long sticks in their mouths for cigars or pipes, and again others had old hats and in one case a dented derby in such a way that it expressed their sense of the ludicrous. Several different attitudes were manifested toward these creations of their imagination. Some called it their teacher and snowballed it. Others fixed it all up with hat, broom, pipe in mouth, and then were afraid of it at night. Again others used the snow figure as a target and shouted with delight when the nose, arm or head was knocked off. One boy is quoted as saying that he was sorry when it melted and left nothing but a puddle of water.

Some of the children made their own forms in the snow by lying down flat and placing their arms out straight. They took great delight in seeing their own forms in this way, as the following quotations will show: F., 8, "Made a picture of myself by lying backwards in the snow." M., 8, lay down in snow to make his print in snow. This effort to see their own form probably arises from a desire to get better acquainted with themselves. Angels were also made by lying in the snow and moving arms and hands up and down in the snow to make wings. Whole rows of these were made side by side. Others made like figures and called them butterflies. Again others made forms of cats, dogs, bears, lions, tigers; drew pictures with stick, and wrote names.

The data do not permit an accurate treatment of the ages of these children but the extreme limits are pretty clearly marked. Of those children whose ages were indicated none were below 5 and none above 13 who participated in making snow men. Of the 82 whose ages were given 6 were 5 years old, 12 were 6 years old, 16 were 7 years of age, 14 were 8, 7 were 9 years old, 12 10 years, 7 were 11 years, 8 were 12 years. It would have been a very interesting thing if the data had been given

to study the development of this tendency to make human forms and its decline. The fact that none of these children whose ages were given continued the making of snow men beyond their 13th year would seem to indicate that this must be near the decline of this form of expression. Indeed it is not surprising that this should be the time when they lose interest in this form of self expression since this is the period of change to larger life and interests. Reason is also more fully developed and children live less in the imagination. This is also the beginning of the awkward age when children have less confidence in themselves and more hesitation in expressing themselves in any form. They are more able to judge between what they actually do and what they attempt in way of self expression.

As has been stated before we have here a photograph of the children's minds such as can be gotten in few other ways. The pedagogical hints and suggestions to be derived from this study touch some of the most vital questions in education to-day. The satisfaction which the child gets from these crude expressions of his ideas emphasizes the fact that he has a craving to express himself, and he should be encouraged to do so. But he must not be required to be too minutely exact and painstaking in his work. The rough outlines satisfy him and are the best his stage will allow him to use.

The general character of the forms made by the children indicates that their early efforts embody types and not copies of real objects or forms. The type does not lead to careful attention to details and so more attention can be given to the main outlines. This needs to be considered in asking young children to draw.

Among primitive people everywhere this expression of types is attempted and the representations have a marked resemblance to those of children.

There is good reason for believing that the child's interest in this form of expression rests upon a strong phyletic background. The earliest records of mankind show that even at that time man had developed a relatively adequate way of representing human and other forms.

The other constructions of snow were numerous and varied, principally forts, houses, caves and large balls. The forts were mainly built of large snow balls piled on top of each other with snow piled up around them. Sometimes they were closed on three sides but more often simply a long, large pile of snow was made for protection in the snow fights. Some made large mounds and dug out the inside and made small windows through which they could watch the enemy. Ninety boys and 79 girls were reported as participating in the construction of

these forts. Besides these many papers mentioned groups of children engaged in these plays. In fact all play of this kind is done by groups of children. The group is the unit in the construction of the forts and in carrying on the contests. The children divide themselves into two groups and each builds a fort and then each tries to destroy that of the other, or to drive them away from it. Sometimes flags are placed on the forts and then the contest is directed toward capturing the flag. There were 67 different descriptions of, and allusions to, these group snow fights. Sometimes there was a captain on each side and definite rules controlling the battle. Sometimes forts were used to hide behind and throw at passers by; again there were a number of descriptions of snowball battles between two individuals at close range and with great vigor. The throwing of snowballs at marks, and without purpose was also often mentioned.

These snow battles and the impulse to throw balls at each other and at passers by indicates a ripeness of the muscles involved in throwing, which are especially the muscles of the shoulder and the upper arm. These snowballs are admirably adapted to give expression to this tendency. They are more or less harmless and yet form excellent missiles. They are sufficiently effective to enable the one or the other of the contestants to triumph by virtue of superior skill and strength and yet not severe enough to do permanent harm. The vigor with which these contests are carried on and their spontaneous nature leave no doubt that they satisfy a deep-seated craving and interest, and are echoes of an age when skill in throwing meant survival. This will be referred to in another connection.

It would be interesting here to see how this tendency on the part of the children is met by the schools. In a number of schools with which the writer is acquainted there is absolute prohibition in the matter of throwing snow balls, or engaging in snow fights of any kind. He has very distinct recollections of one school of 300 pupils in which such a rule was enforced. The falling of snow always brought its troubles, and frequently after the school intermissions the halls of the building reverberated with the reminders that were being applied to the effect that the wicked urchins should not again give expression to this phyletic tendency. Perhaps rightly, too, for the limited area of the playground and the proximity of dwelling houses all around made it rather dangerous to allow the free throwing of snowballs. But this lack of school ground only goes to show that school officials still have the idea that schools exist for the sake of book learning and that the physical part of the child is of secondary importance. The idea that the child must first have a good strong body and that the school should work towards this end has yet not taken firm root.

The snow houses, caves and tunnels were more or less elaborate; 106 descriptions of houses were given and 84 of caves and tunnels. The houses were built by piling up snow and packing it down, digging out the centre of snowdrifts, or by rolling large balls and piling them up. A few quotations follow: M., 10. "Built snow house and put cat in it and closed door for ten minutes." M., 8. "Built snow house which held 5 boys." 4 M's. "Built house by first piling up snow then dug passage through it and then dug out inside; poured water over it to freeze it." M., 4; F., 8. "Built house by digging in deep snow bank, then filled entrance except door, then took carpet, stool and table in and ate inside." M., 9. "Built snow house against fence, could crawl in at one door and out at other; this was pleasant." F. "Built house by digging out room and putting chairs inside, had party inside; did not mind cold for we bundled up." M., 5 and F., 9. "Built snow up 4 feet high, patted it down with shovel dug out inside, sat in it with candle and ate like Eskimos." One thing that interested the children much was to make long tunnels into a snow bank and then dig out a large space in the interior of the bank and crawl back and forth. The opening into the interior must be small so as to be just large enough to crawl in and out. This gave special satisfaction as was also noticed with the cave houses.

One hundred and twenty children liked to roll large balls. A variety of reasons was given for this; some wanted to see how large they could make them, to see how long it would take them to melt, to see who could make the biggest, to roll them down hill, or just because they liked to make them.

It may seem strange to speak of snow as a factor in education and yet the opportunities here are simply unlimited in the northern states where there is a liberal supply. For purposes of construction and moulding there is no better material and it is evident that children enjoy this. If instead of prohibiting the play in snow, as is now so frequently done, children were encouraged to play with it and to construct objects, roll balls, mould figures and have snowball contests, it could be made a valuable help in education.

STRINGS

What part do strings play in the early life of children? What string games and plays interest them? Perhaps few persons fully appreciate the large and varied use which children make of them. Of the five hundred children reported upon, not one was there who did not have some interest in strings. The infant of two or three soon learns to tie chairs together, to pull objects attached to strings, or to play horse with strings for lines and harness. At this age the string is

also used to collect buttons and beads, which are used for decorative purposes and fastened to the hair, or worn around the neck, or wrist. As the child grows older these uses are extended in many ways. The material used varies from the common cord to willow bark, corn husks, grasses, leather strings, and rope.

A differentiation in the use of strings soon becomes evident between boys and girls. Although a great many uses are common to both and what one does the other imitates, yet by the time they reach the 9th or 10th year the different interests are pretty clearly marked. Some of the uses common to both are making cat's cradle, see-saw, playing horse, tying strings together, pulling objects with strings, making telephones of strings and cans, and making animal forms. Both boys and girls braid but with different ends in view. The most common mode of weaving is done with the use of a spool and four pins or tacks placed at one end of the spool to which four strings are attached. The strings are then braided or woven with the use of some sort of needle. The most common modes of braiding are also practiced with from three to seven strands. Grass, hay, willow bark, corn husks and silks are used in braiding, besides the common cord. The most frequent use of the braided material is that of lines for playing horse. The girls very frequently use the braids to make circular or other shaped mats. They also braid rags and make rugs of them. Hay, grass, clover stems, or varied colored strings are braided and from these are made necklaces, watch chains, bracelets, or rings; corn husks and silks are braided and used for doll's hair. The boys use the braided cord, grass, straw, leather, or willow bark, for harness, whips, fish nets, swings, to make bows or as belts for machines. Boys have a greater interest in saving string and when they find a cord of any kind it usually finds its way into their pockets. Both boys and girls are interested in knots of various kinds; but here again their interest is different. Boys want to learn to tie hard knots and knots that others cannot undo, such as puzzle knots, sailor knots, slip knots, twists and hitches. Girls, on the other hand, are more interested in knots for decorative purposes. They like to tie many knots or loops of various kinds in a string and hang it about their dress, or they may even have knots take the place of beads around their neck, etc.; or they may knot a hammock for their dolls.

Some of the applications of strings which belong more distinctly to boys are making bows to shoot, sling shots, flying kites, making fish nets, rigging up machinery with strings for belts, devising new puzzles and tricks of various kinds, rolling

strings into balls, and making harness for dogs or goats. The tricks with strings, such as working out puzzles; cutting a double string and apparently chewing the ends together; or tying a string to a button-hole and taking it off without untying it, have a great charm for the boys.

Crocheting, working with the needle, and making ornamental articles such as bows or loops, appeal more especially to girls, according to the returns, as is illustrated by the following:

F. at 11, had fancy for tying bows; fixed odd bows, on back of chair, in my hair, around my waist, and for my dolls.

F., 8, decorated her doll's hats with ribbons, sashes and ties.

F.,—, made rope dresses, decorated myself with rope in which were tied knots and loops.

F., 7, braided colored string into bracelets, necklaces, and rings.

At about 6 or 7 the girls use strings to sew dolls' clothes and make play clothes for themselves. Later, at about 10 or 11, the use of the needle appeals strongly to them. The various stitches, such as the cross-stitch, hemstitch, brier-stitch, feather-stitch, knot-stich, backward stitch, cat-stitch, crow-stitch and button-hole stitch, are learned and new ones invented. Crocheting is also learned with enthusiasm and new patterns are puzzled out and named. Laces of various patterns are made and new ones attempted. This interest in the various lines of crocheting and needlework may grow and continue through life.

There is perhaps no spontaneous interest on the part of the child that is more marked and definite than this string interest. It has not been adequately recognized and made use of in the child's education. Nothing which the child could do would give it a better idea of man's early difficulties in overcoming his environment than this free use of the string. It could also be made an excellent means of introducing the child to many phases of the great modern industrial world. The extended application of the principle of the string underlies much of the progress of civilization. What would be left of our industries or even of our civilization if all applications of the string were never invented? Not only would all textile industries disappear but also all such machinery in which belts and chains are used. The early steps in civilization seem to have depended as much upon its use as those of later times. One need only visit an anthropological museum, such as the Peabody Museum in connection with Harvard University, to be convinced of the great part played by the string in the early struggles of the races. The great degree of perfection of many of these uses of the string found

among primitive people indicates that its use came very early in the evolution of man. "The textile art is older than the human species. For not only spiders and many caterpillars draw out extremely fine threads, but birds wove nests long before the advent of man on earth. . . . There is no reason to doubt that the very first women were weavers of a crude kind, and that textile art has been with us always in one form or another." (The Origin of Invention, O. T. Mason, p. 224, chap. VII.)

Most primitive people use the string in connection with religious rites, ceremonies and magic. Many of our common string games are hoary with age, and were perhaps once connected with magic performances, of various kinds. For example, our common game of cat's cradle is made in some form or other by nearly every primitive tribe of the world to-day; and is also known by most civilized people. It has been found among the people in the following countries: Korea, Japan, East Indian Archipelago, Australia, Africa, western Asia, and among the Eskimo, American Indians in both North and South America, and the people of western Europe. There are 97 varieties of this game known to the world. Some are accompanied by muttering chants or songs. In other cases a consecutive story follows each movement. The same history might probably be worked out for each of our common string games and tricks. "All over the world strings, cords, and knots enter largely into the magical practices." (A. C. Hadden, Introduction to String Figures, p. XXIII.) The fact that primitive people make so much use of the string in all phases of their daily lives, including their religion and magic, gives some idea of the large part which it must have played in their struggles and how much they valued it. It undoubtedly became of survival value to many primitive tribes.

The analogy between children's uses of strings and those of primitive man is thus seen to be very close. Undoubtedly many of the uses which children make of them to-day are given us by social inheritance as is generally maintained. But the aptitude which children show in their use and the intense interest with which they play with them, and even save them, point to something more than mere imitation. It has been said that the younger the child is the racially older it is. If this be true it can readily be conceived how the early struggles of the race with the string as a means of evolution, could give to the modern child's mind a psychic stringward tendency. From this point of view it becomes all the more necessary to utilize this racial instinct in the early education of the child.

POINTS AND EDGES

The first fact which the returns reveal is that girls are much less instructed in the point and edge as a means of sticking and cutting than the boys. Of the 384 children reported upon, almost three-fourth are boys. The knife leads in interest, nearly every boy being keenly anxious to own a knife of some description. If unable to own one he gets much satisfaction in using some one else's. The interest begins to manifest itself at about 5 years, and grows to about from 12 or 15, when it is at its height. Considerable sacrifice is sometimes made by the boys in order to purchase a knife. Several instances were given in which boys worked for several days for a knife or deprived themselves of other things to buy one. Some spent all their money for knives owning as high as five or six at one time. Their interest centres in the number and sharpness of the blades. Each boy's knife must be sharper than those of others, and comparison of knives with respect to sharpness is common. The blades are tested by cutting into hard material, by seeing which can cut the largest shavings or chips from wood, or by trying to cut paper or a hair. This leads to sharpening the blades and all sorts of efforts are made to improve this quality. The grindstone and whetstone are freely used.

Boys not only like to have sharp knives but they want to use them. They whittle sticks, boards, fences, palings, boxes; they cut into furniture, desks, chairs; they carve their initials upon everything which will receive them, including desks, buildings, fences, trees, walls, stones and cliffs. Pencils are sharpened often and to fine points, even when not especially dull. They also carve objects out of wood, such as canes, chairs, whistles, boats, boxes; cut out baskets and other toys from English walnuts or peach seeds. They throw their knives with open blade so that it will stick in trees, boards, or the ground. They play mumbly-peg. They often make motions with open knife in hand, brandishing it as if to stab some one. They sharpen sticks and call them daggers, or whittle out swords with which they have Indian fights, or cut down tall weeds as enemies. Stiff weeds are cut, sharpened at one end and hurled endwise at objects. Great skill is sometimes acquired in this way and the consummation of a boy's ambition is to be able to hurl one of these instruments through a pumpkin or watermelon. The sword, spear and dagger appeal strongly to the imagination of the boys. They fasten these to their belts and play robber, or soldier, and sometimes impersonate great characters like Washington, or an Indian chief, and kill imaginary enemies or charge upon wild beasts

such as mullen stalks, stumps, grape vines, and bushes. One boy of eleven carried an old butcher-knife with him and made a spear by fastening a machine guard on the end of a broom handle, and a tomahawk by fastening a machine section on the end of a stick. Another had a sharp-pointed wire for defense, and a tin sword, which he loved to carry. These weapons are carried about or hung up in their rooms as decorations. Old swords are especially prized for this purpose, and are placed in conspicuous places. The bow and arrow are made and used with some degree of skill. The arrows are sometimes tipped with some metal point, as a nail sharpened to a fine point. These are shot at marks and sometimes at cattle, hogs, dogs and cats. Another practice which seems to give the boys an unusual degree of pleasure is to put a pin in the toe end of their shoes and prick their companions, or the cat or dog, with considerable degree of force. Sharp tools, such as the hatchet, ax, saw and chisel, are used with no definite purpose but with interest. Sharp sticks and sharp stones make a strong appeal to children, and many instances are given of collections being made of these. Sharp stones especially are picked up and saved or used for cutting or scratching, or they are thrown at a board and the effects examined. Several cases were given that might be considered as verging on the abnormal. One girl could n't look at a sharp dagger without wanting to use it on some one. Another case was that of a boy who had a mania for pricking other boys until he drew blood. The report states that he gave much trouble. One boy of 6 sharpened a stick and speared bugs and worms.

Girls are interested in the use of the scissors and love to cut designs of paper, or cut out paper dolls, or fringes in cloth or paper. Some use a sharp stick for a pencil to write their names in dirt and snow, or draw figures or forms. They also use the knife to whittle and cut their initials on trees, fences, or other places which will receive them.

The analogy between these activities of children and those of primitive man is very close. The point and edge were both tremendous factors in man's early evolution. Among nearly all primitive tribes on earth to-day the point and the edge are developed to a high degree of perfection. We must assume that there was a time when neither of these existed in any form. There must then have come a time when primitive man learned the effect of bringing his whole strength to bear upon a single point. This secret, whenever and however it was discovered, gave the favored tribe great advantage both in their quest for food and in offense and defense. Close upon this must have developed the use of the edge in some form or other; or perhaps it came before the point. At least it does

not seem illogical to associate them somewhat closely in their origin. With these weapons the tribe would be almost invincible and the cultivation of their use would become a matter of greatest moment. All through man's history they have played an ever increasing rôle. In the complex world of to-day with its myriads of inventions it is perhaps difficult to imagine that the point or the sharp edge should at one time have been the great means of survival. And yet this in all probability was the case. From this point of view it is not unlikely that those members of the tribe survived who had most skill in the use of these instruments; and that those tribes survived who were favored by these skillful individuals. In short, the use of these instruments led to somatic and psychic modifications of the race. In the story of psychic evolution some such sequence of events must have taken place, and from the point of view of the child it offers the only satisfactory explanation of this unusual interest in the instruments of this character.

The strange phobia of sharp objects with which the insane are sometimes afflicted would hint a similar origin. What in normal minds appears only as an interest is here magnified to great proportions and causes the phobia.

Whatever may be thought of the origin of this interest on the part of children in the point and edge, its use as a means of education cannot be ignored. Here again we must move with the current of the children's natural bent and allow this to decide what should be done. This factor is too often neglected in planning manual training work and other exercises for children.

MODIFICATION OF BODILY FORM

The facts brought out by the returns on the subject of modification of bodily form by children indicate that this is attempted to a very marked extent. Of the 245 girls and 126 boys reported upon, practically all tried to change in some way their features, or stature; or they tried to use their dress to add in some way to their effect on others. This is very largely due to a growing consciousness of self. The child very early becomes acquainted with the different parts of his head and body, but not until several years later does the sense of self assume a critical attitude. The child of two or three knows that he has a nose, eyes, mouth, ears and hair but he does not find fault with them. But at six or seven, and especially a few years later, he begins to examine his features and dress in the light of his more critical observation of the features or dress of others; or of what he has heard others consider attractive. The features and dress of persons whom the children admire

become standards with which their own are compared. There seems to be much more of a tendency on the part of girls to do this than on the part of boys.

The nose is one of the features frequently referred to and is subject to great criticism. There is, however, no definite standard of shape or size. Some think their nose too broad and pinch it constantly to reduce its width. They even put clothespins on it at night and try to sleep with the nose in this position. Others put clothespins on the nose to make it pointed. Again others want a pug nose and hold it in that position for long periods at a time. A few quotations will, perhaps, help show their attitude:

F., 20. At ten I pulled my nose so it would be pointed like my aunt's.

F., at 9. I pushed up my nose to make it shaped like a girl's I admired.

F. Pinched my nose so it would be like my teacher's.

F. Slept on my nose to keep it from being hook billed.

F. Nose turned up, wanted it to turn down. Thought of cutting off end, it was too long.

Dimples are another element of beauty, the absence of which is lamented. Several rather strenuous efforts to secure these were reported. One girl stood before the mirror for hours and pressed her fingers into her cheeks in the hope that dimples would remain. Another put clasps, which her brother used on his trousers when riding a wheel, on her cheeks. Still another made faces before the mirror to induce them to appear.

The ears also receive their share of attention. The majority of those who mentioned their ears wanted them pierced so as to wear rings in them. Some begged to have this done; others tried to do it themselves. One group of girls retired to a secret place to attempt this but their courage failed. All sorts of substitutes are made for ear rings, such as fastening strings with something attached to their ears or sticking maple seeds on their ears. One girl of 13 pulled the lobes of her ears so they would become longer. Although boys do not pay much attention to the shape of nose they are somewhat concerned about their ears. One boy of 11 wore nightcap to train his ears back; another of 12 tied a towel around his head to keep his ears from growing too large.

The eyebrows and eyelashes are considerably mutilated and for various reasons. Some children pull them out entirely, or burn them off to see how they will look without them. This is especially true with boys. Others do so to wish with them. Sometimes they are trimmed in the hope that they will grow, or because they seem too long. They are also colored or pencilled, one girl of ten putting shinola on hers.

The hair is perhaps subject to modification more than any of the other parts of the head. The scissors are freely used by both boys and girls, and they cut their own hair even at four or five years of age. They cut their curls off because they are tired of them, to see how they will look, to improve their looks, and one little girl cut hers off thinking she would then be a boy. A number of girls wanted curly hair and resorted to many means of accomplishing this, such as soaking it in buttermilk or sour milk, putting mucilage in it, or fastening shavings in it. The color of the hair also causes some anxiety and attempts are made to change its color. One girl of 10 who had red hair and freckles exhausted all her remedies in trying to change the one and remove the other. The manner of wearing the hair is also considered of some importance among children, especially girls. Some braid long ribbons into their hair to make it look longer; others put it up on top of their heads to look taller and older. One girl of 9 took great pleasure in wearing her hair in various ways, changing it frequently.

The complexion of the face is changed in many ways. Girls from 8 to 14 paint their face to improve their appearance, and give especial attention to the cheeks and lips, and use starch, water colors, young oak leaves, mullen, snow, candy and pink chalk. The mullen and snow are used to rub the cheeks briskly. Boys also color their faces, but usually for purposes of disguising themselves; or to play Indian, or impersonate some character. Girls also color their faces for purposes of impersonation. They not only color their faces but dress up in all sorts of ways. The dramatic instinct is strong and expresses itself in many ways, as will be noticed by the following quotations:

F., 9, dressed like my grown sister, powdered face.

F., 8, dressed up, using long skirt, high hat, put hair on top of head with ribbons.

M., 11, dressed in Indian costume, hat-band with feathers, belt with daggers, cheeks painted red, black lines under eyes and on forehead.

M., 10, blackened face and put old clothes on to scare sister.

There is also a marked tendency on the part of both boys and girls to impress initials, pictures, or marks of various kinds upon their hands, arms and face. An indelible pencil may be used or tattooing may be done with pin and ink. Pictures are transferred to the arms or hands and care is taken that these pictures, marks, or initials are not washed off for a long time. They are marks of distinction, and great pride is taken in showing them. This is especially true of boys who

have figures, stars, or anchors, tattooed on their arms. The finger nails are painted, pointed and notched in peculiar ways, and a favored nail may be allowed to grow long and be protected with considerable care. The girls are sometimes solicitous about the size of their hands and the shape of their fingers, and they are squeezed to make them small and of proper shape. This attitude is sometimes taken towards their whole body. One girl lamented her small stature and asked to be lifted by the head, while another sat much because she thought it would prevent her from growing tall.

It seems hardly necessary here to mention the analogy between these activities on the part of children and those of primitive man. There is perhaps not a single trait here mentioned that could not also be found among savages. The awakening of the sense of self in primitive man through long ages, is probably duplicated in a somewhat telescopic fashion by each child.

The one thing which such a study as this reveals is that children have their own point of view, many times radically different from that of the adult. One must first try to understand children and get their point of view if one is to treat them sympathetically and be helpful to them. Pedagogy sins more just at this point than perhaps in any other way. Many a parent may love his child and want to do all that is necessary for its welfare and yet utterly fail because of this inability to see things from the point of view of the child's psychic evolution. The same might be said of teachers.

ATTITUDE TOWARDS CLOTHING

From the returns, one must conclude that Whittier's "Barefoot boy with cheek of tan," tells only half the story, and that the barefoot girl with cheeks of tan is also to be found. Of the 350 children reported upon almost two-thirds were girls, and there was not any less enthusiasm among them for going barefoot than among the boys. From about four or five to 12, and even later, the desire to go barefoot seems to be universal among both boys and girls, although they are not all allowed to do so. This feeling is strongest in the spring when the days begin to turn warm, and just after a rain. The first hint of warm weather seems to awaken the desire to go barefoot, and in many cases parents can scarcely induce their children to wait until the days are warm enough. One boy of 12 wants to go barefoot in spring as soon as frogs croak. All sort of excuses are made by the children to accomplish this end. Some go away from home and indulge in this for a few minutes when parents do not know it. Others take off their stockings and put their shoes back on. Those children who

live in the city and are not allowed to go barefoot but who have relatives in the country, where they are permitted to do so, regard this as of sufficient reason to visit these relatives in the spring and summer. A number of teachers reported that in the spring when the days begin to be warm, every child in school would often be found with his or her shoes off at noon, and would run about in greatest glee. Several reports were given of boys taking off their shoes in winter and running about in snow for a short time. The desire to run about in the mud and wet grass with bare feet becomes very acute after a hard rain. Children say that the mud feels so pleasant, and the cool, wet grass makes their feet feel comfortable. Boys like to go bathing, or swimming as they call it, and run about on the sand in their bare feet. Others like to strip off clothing and run about in the rain. Small children seem to take special delight in running about after their bath.

The desire to go without a hat or bonnet seems to be almost as strong as the desire to go barefoot. Many cases were reported of both boys and girls going bareheaded all summer and until late in the autumn. Even when parents were solicitous about their complexion and tried to induce them to wear sunbonnets they refused to do so. F., up to 9, resented sunbonnet which mother tried to compel her to wear. M., 7, quarreled with his mother for weeks because she tried to induce him to wear a sunbonnet or large hat. He preferred to go bareheaded. F., 6, M., 8, never wear hats. These citations could be indefinitely extended.

This attitude on the part of children is strikingly analogous to that of primitive man; and it is difficult to explain on any other ground than that of atavism. It seems to be another evidence of recapitulation on the part of the child.

STRIKING

The striking propensity of children from infancy up to the age of 12 to 14 is so universal that it has impressed itself upon all who have even casually observed children. It has often been termed the age of striking and all through child literature appeal is made to it. The pounding of the small child upon the plate, table, chair or floor is proverbial. A boy with stick for sword and a mullen stock or flock of geese as enemy we have come to associate together. It would have been strange, therefore, if the returns had not borne out this idea that is so common concerning children. Ninety per cent. of the answers to the question concerning this phase of child activity confirmed the above prevailing notion. The infant is handed a spoon or toy and at once it begins to swing it about. The grasp is firm and can easily hold the object; finally it strikes

the table or plate; this is repeated over and over until the association is formed and then the striking becomes continuous for long periods at a time. Numbers of instances were given of this continued repetition of one act. The returns indicate that this begins not later than the sixth month. This striking by infants of table, chair, plates and the floor with toys, spoons, knives, forks, blocks was mentioned in practically all the returns. Soon the stick comes into use. Those who suffer most from this are toys, the cat, the dog, other children and adults. This tendency increases, especially with boys, from 5 to 10. Whips are made from straps and sticks or are cut from bushes and carried by the boys who constantly strike at everything that comes within reach as the following quotations will show: M., 7, carried club, stick, or cane for gun. Loved to play school and be teacher so he could whip. M., 7, struck trees and fences as "Jack the Giant Killer." Played policeman and carried club. Boys of 8 lined up with sticks and then charged upon weeds and bushes. M., 9, liked to play horse with whip and use it. Liked to hit weeds, stalks and stems. Boys of 9 struck cat, dog, trees and house with stick. M., 8, hit himself with whip as horse. M., 10, likes to cut tops of grass off with stick. Similar statements were repeated over and over in the returns. Many of the boys and girls say they carry a stick because they feel safer when walking along the street or road. In walking along with a stick it may be used to strike at weeds, animals or even people. It is also dragged along a picket fence so as to make a rhythmic noise. In riding in a vehicle one great pleasure is to use the whip on the horses. Boys and even girls engage in whipping contests, in which two of them with whip in hand beat each other until one or the other gives up. Driving stock also gives great pleasure because the whip can be freely used. Much damage is sometimes done in striking flowers, small trees and other plants of value. There seems to be an almost blind impulse to perform this motion with something in the hand in the shape of a stick or whip.

From the use of the stick and whip the child in the course of time learns the use of the lever. This usually does not occur until the tenth or twelfth year. There is considerable variation here. It is interesting that many children do discover this incidentally in their play with the stick.

Throwing probably develops in the child from striking. In his effort to strike, the object in his hand may slip from his grasp and fall to the floor. In the course of several repetitions of this the child learns to drop things on the floor and from this develops the idea of throwing. The motion is at first awkward and is little more than an aimless motion of the

arm and hand even up to the third or fourth year. Progress is slow; but gradually through constant practice and continued repetition such as only a child can engage in, the arm and shoulder muscles come under control and the act of throwing becomes more definite and purposeful. This instinct to throw grows with the child. The returns show that from about the first year up to the 12th year the interest grows and is at its height at about 13, although in some cases it goes beyond that. There was unanimous testimony on this propensity of the child for throwing. First girls and boys both throw with almost equal interest but in girls it begins to decline very soon. Small children throw everything they can get hold of: blocks, toys, spoons, knives, forks, food, balls, etc. When larger they throw stones, skippers, snowballs, water soakers, apple stealers, corncobs, and pieces of coal. Stones are used far more than any other object in throwing. The returns show that they throw at trees, street lamps, birds, boys, horses, wagons, people, windows, church steeples, signs on street, shutters, passing trains, brass balls on top of poles, cats, dogs, fruit in trees, chickens, and barns. About 40% of the boys threw at cats, dogs and birds. One of the returns gives an account of a boy with a stone in his hand slipping up to a pigeon walking about in the street and throwing at it. He struck the pigeon on its leg and broke it, and as the pigeon flew the leg dangled in the air; as the boy saw this he shouted with delight. Animal targets seem to give more pleasure than inanimate ones. This is especially true when snowballs are used. Great numbers of instances were given of boys hitting adults with balls. One lady indignantly remarked that the boys had become a nuisance and that she could not pass along the street without being pelted with snowballs. There were several accounts given of boys who used stones to fight with each other with more or less serious effects. The sling shot was mentioned at least 100 times, as a means of throwing stones. It could not be ascertained whether the boys ever invented one or not but that it appeals very strongly to the boys is quite clear.

Throwing contests are frequent and distance and accuracy are both aimed at. Throwing across a river, over a tree or building, or at some distant object tests the force of the effort. Various objects are used to test the accuracy of the throwing. There are also a great variety of games with the ball which involve throwing. These appeal greatly to boys, and as we know may develop into a permanent interest and lead to continued practice all through youth and young manhood.

The striking and throwing instinct and tendency of the child is out of all proportion to that which it would be if

it were a mere matter of imitation. Thus we must go to the early development of man for a key to the problem. Both striking and throwing involve mainly the racially old and fundamental muscles of the shoulder, arm and trunk. Both must have developed early in the life of man.

After our ancestors had descended from their arboreal life and took their erect position on the ground they had the use of their upper limbs for purposes other than those of locomotion. This enabled them to devote their hands to use in their quest for food and also more directly for purposes of attack and defense. The life in the trees had already developed the hand to some extent, and now the use of the hand and arm were constantly brought to bear in the daily life of the individual. With the free use of the hand it is not surprising that in the course of time our ancestors should have come to hold a stick in their hands, and, by swinging it, strike it against the earth or some other solid object much as an infant would do under similar circumstances. As in the case of the infant this might have to be repeated a number of times before its full significance dawned upon the individual. If, as Lotze says, we instinctively project our sense of self to the end of whatever we handle, such as a stick or other object, it can readily be seen how this primitive being would come to have a sense of increased length of arm. The force and effectiveness of the blows which he could deliver would also be very evident to him. The advantage of the use of such a stick or club was almost incalculable to the user in the struggle for supremacy with animals and perhaps other persons of his or other tribes who had not yet learned the use of this weapon. It might even be compared to gunpowder as a means of conquest and civilization. It insured food with greater ease and therefore gave leisure to fight. This emboldened the tribe with this advantage, to enlarge its domain and seek new regions for food. This led to new dangers from wild beasts and other foes and so put the favored tribe to greater and greater tests, and thus weeded out those less gifted in the use of the new instrument of warfare and left those most skilled in its use. In this way through the process of selection and the survival of the fittest the good strikers remained and handed their inheritance to after generations. The ages that must have elapsed while even such a simple thing as the use of a stick or club evolved to a high degree of perfection, must have been long; thus this power could easily become a real means of selecting those who were best fitted by virtue of bone, muscle, and brain centres to survive, and of weeding out those not so well adapted to this means of offense and defense. Again, some genius of the tribe may have modified and im-

proved the original club, and as the generations went by there were evolved all primitive means of striking, such as the ax, the hammer for crushing food, grains, nuts; the sword and other "instruments of warfare, industry and even sport."

That this long process of evolution, extending over a great period of time, should have left its stamp upon the soma and psyche of the race and thus upon modern childhood and early youth can hardly be doubted. We thus have at least a tentative explanation of this impulse of the child to strike anything and everything with such satisfaction, and of the youth who is never so happy as when he has a stick or whip in his hand striking real or imaginary objects.

As it was with striking so was it with throwing. There seems to be little doubt that this developed in the race as it does in the individual, namely from striking. In either case the only thing necessary to lead from striking to throwing would be that the instrument used for striking should accidentally leave the hand of the user while in the act of striking. Again, as in the case of the child, this might not teach its lesson the first time. But finally it would force itself upon the notice of the user and the force with which it would leave his hand and strike some object would finally be grasped. Here would be revealed the new power of bringing about results at some distance from the person throwing.

This again must have given new power for conquest that we can hardly overestimate. Here, again, perfection of the art meant new conquests, new dangers, and the application of the law of the survival of the fittest, and the handing down of this inheritance to after generations. Thus the tendency of the child to-day to throw balls, stones, snowballs, apples and anything else that he can get hold of, has its origin in these past ancestral experiences. The lovers of base ball may even be indulging in and expressing ancestral echoes.

It is a pretty well-established fact "That the fixedness of a tendency is roughly proportional to the length of time during which it has characterized the race." (Tyler, in the "Man in the Light of Evolution," p. 139.) The slow progress which our ancestors made gave this tendency an abundant opportunity to become deeply engraved upon the brain centres. This is illustrated in the animal world by "The shepherd dog and the bird dog brought up in the house apart from sheep and birds, who went the one after sheep and the other after birds as soon as turned loose in the fields. The stimulus of the appropriate object was all that was necessary to arouse the slumbering inherited instinct in the brain." (Tyler, in the "Man in the Light of Evolution," p. 137.)

The value of this point of view in its bearing upon educa-

tion is evident and has been frequently emphasized in recent years. What is needed most of all to-day in the educational world is a proper perspective of child development. Biology has given us a pretty definite outline of the evolution of the body in that it has discovered the different stages of growth through which it passes, from conception to maturity. This needs to be applied to the psyche and clothed with flesh and blood and made a living, breathing reality; for the psyche, too, has these stages which express themselves spontaneously if given an opportunity to do so. Tyler says, "The interests of the child are as truly symptoms of the attainment of a certain stage of development and real needs as the craving of the legs for exercise or hunger of the body for food," and if the stages of the soma and psyche are not given an opportunity to have their normal course they are likely to become the source of imperfections in adult life. We must come into phylogenetic rapport with the child if we are to undertake to guide his physical and mental growth successfully. It is becoming more and more clear as the child study material accumulates that the child has feelings, motives, instincts and interests that should guide the educator in his work rather than that the educator should undertake to direct and modify the child's development. The child must be allowed to evolve naturally and in harmony with its racial inheritance. But in the school work of to-day the social inheritance of comparatively recent times continues to be imposed upon the child and the deeper impulses of its soul are scarcely touched.

The composite picture of the children's activities given above tend to be misleading so far as the individual child is concerned. Perhaps no single child actually engaged in all these activities. But it is just as true that every child has more or less marked dormant impulses to do all these things, and would do them, if the environment were such as to encourage their expression. Here, then, is the great and crying need of the child. It is needless to say that there are hundreds of other vague instincts, motives, and interests in the child's soul besides those above referred to. The full and complete expression of these would give every child a richness of mind that would characterize it all through life and enlarge its sphere of interests to an extent hardly dreamed of now.

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THE MEASURABILITY OF ATTENTION BY PROFESSOR WIRTH'S METHODS

By L. R. GEISSLER

In a recent number of the *Psychologische Studien*, Professor Wirth publishes an article entitled "Zur Messung der Klarheitsgrade der Bewusstseinsinhalte,"¹ which is a reply to my "Critique of Professor Wirth's Methods of Measurement of Attention."² The Reply consists of twelve sections, consecutively numbered; and the reader naturally infers that it presents twelve different objections to my Critique. As a matter of fact, the contents of the sections are not sharply differentiated, and the argument of one is sometimes repeated in others. Hence I shall not attempt to take them up in order, but shall merely refer in parenthesis to the particular section in which the point under discussion receives the greatest emphasis. I shall, furthermore, confine myself to the four articles mentioned in my previous paper.³ Wirth's book, "Die experimentelle Analyse der Bewusstseinsphänomene" (§ 1), arrived here after my manuscript had gone to print, and I was, therefore, unable to reply to it,—although this explanation may seem inadequate to my opponent, who devotes more than a page to my work on "The Measurement of Attention" some half year before its appearance⁴ (§ 6 and § 10). I do not deny that Wirth's book clears up some of the obscurities here mentioned, just as his Reply is in some respects an illuminating and welcome commentary upon his own work; but these facts do not affect the validity of my original criticism. I am sure, too, that my article contains nothing to justify the personal attacks scattered throughout Wirth's pages. The charges are not only absolutely untrue to fact, but they are also unsuitable for public discussion and refutation. I shall make no reference to them in what follows.

The keynote of Wirth's article is the complaint that he has

¹ *Psych. Stud.*, V, 1909, 48-72.

² *AM. JOUR. OF PSYCH.*, XX, 1909, 120-130.

³ Zur Theorie des Bewusstseinsumfanges und seiner Messung, *Phil. Stud.*, XX, 1902, 487-669; Die Klarheitsgrade der Regionen des Sehfeldes bei verschiedenen Verteilungen der Aufmerksamkeit, *Psych. Stud.*, II, 1906, 30-88; Die Bestimmung der Aufmerksamkeitsverteilung innerhalb des Sehfeldes mit Hilfe von Reaktionsversuchen, A. Kästner and W. Wirth, *Psych. Stud.*, III, 1907, 361-392; IV, 1908, 139-200.

⁴ *AM. JOUR. OF PSYCH.*, XX, Oct., 1909, 473-529.

been entirely misunderstood. The possibility of misunderstanding I frankly admitted; Wirth's language, I said, is "both difficult and obscure," and is "not always consistent;" and indeed, any one can verify the point for himself by trying to translate some page of Wirth's writing into another language. I, therefore, offered my "interpretation of Wirth's attitude . . . with all reserve," and I am now merely repeating a former statement when I say that I was in constant doubt as to the very nature of his main problem (§§ 1-5). Nevertheless, in my discussion of Wirth's results, I tried to keep the two possible forms of his problem equally in mind, and to remember that his intention might be to measure either the range of consciousness or the range of attention only. Hence Wirth's principal accusation, that I have simply and completely misunderstood him, is incorrect. I may complain, on my side, that in some points at least he has misunderstood me, and has interpreted my remarks in such a way as to exaggerate, or even to create, difficulties which did not exist either in my words or in my thought. Moreover, he often criticises me, without reason, rather for what I have not said than for what I did say.

Thus, with an experimental arrangement as complicated and a method as elaborate as his, it was impossible for me to do justice to all details in a short discussion. For I did not, as Wirth puts it at the beginning of his Reply, intend my article to be "eine Orientierung über den gegenwärtigen Stand der experimentellen Analyse der Aufmerksamkeit."¹ I did not even offer an exhaustive review of Wirth's own work, but merely "a discussion of his contributions to the subject."² Again, Wirth's interpretation of my sentence: "thus the results confirm and extend those of previous tachistoscopic experiments upon the range of attention," implies that I had written: "of *his* previous experiments, etc." (§ 5).³ His statement that I had failed to see the logical relation of the "entweder—oder" (§ 5) in Wundt's discussion of Wirth's tachistoscopes, I can only characterize as amazing. As a matter of fact, I had never even hinted at a reference to that particular phrase. I simply mentioned Wundt's belief that the tachistoscope may be used for measuring not only the range of attention, but also the "Gesamttumfang des Bewusstseins."⁴

¹ *Psych. Stud.*, V, 48.

² *AM. JOUR. OF PSYCH.*, XX, 120.

³ *Ibid.*, 57. For the same reason his objection to the word "extend" falls to the ground. Another obvious case of misunderstanding is the Footnote, p. 61 of his Reply (§ 8).

⁴ *Physiol. Psych.*, III, 1903, 358 f. "Je nach der Anwendung dieser

But the climax of Wirth's misrepresentation is found in his objections to my discussion of his reaction-experiments (§ 12). I did not, it is true, mention the fact that their purpose was purely negative; that is, that Wirth meant to compare reaction-times (which have frequently been assumed to express variations of attention) with the results obtained by his previous method (the *Schwellenmethode*), in order to show that they do not afford as reliable a measurement of attention as do his liminal clearness-values. But then he himself makes no such explicit statement, either in the form of a problem or in that of a conclusion. The only passages which hint at any purpose, with regard to the reaction-experiments, are the following: "da es uns vor allem darauf ankam, die Resultate auf die früheren Klarheitsmessungen nach der Schwellenmethode zu beziehen," etc.;¹ and "wenn irgendwo, so scheint nun gerade für die Abhängigkeit der Reaktionszeit vom Klarheitsgrade der Motivauffassung, *die wir hier im einzelnen untersuchen wollen*," etc.² From these two incidental passages the reader is left to conclude for himself what the "entscheidende Hauptpunkt" of the investigation is; namely, as Wirth now says in his reply, but had not said before, "dass die Methode der (sensoriellen) Reaktion an der Hand der unter vergleichbaren Bedingungen gewonnenen Resultate nach der *Schwellenmethode*, *die allein für uns als Klarheitsmass gilt*, daraufhin kontrolliert werden sollte, wie weit sich die Klarheitsverhältnisse auch in dem Zeitwert widerspiegeln."³ This implication was not observed by me, nor seemingly by A. A. Grünbaum,⁴ who lately published a more elaborate review of Kästner and Wirth's reaction-experiments. For Grünbaum the problem of the investigation is simply to find out "wie das Klarheitsrelief, das durch verschiedenartige Verteilung der Aufmerksamkeit erreicht wird, sich in den Reaktionszeiten widerspiegelt." There is here no mention of a proposed comparison with the results of the *Schwellenmethode*. Nor does this problem occur to Grünbaum when he actually compares the results obtained by the two methods. He merely says: "man sieht leicht,

verschiedenen an dem Apparat möglichen Versuchsweisen kann man denselben entweder, ähnlich wie das Falltachistoskop Fig. 339, zu Bestimmungen des Umfangs der Aufmerksamkeit, oder aber auch zu Versuchen über den Gesamtumfang des Bewusstseins verwenden," etc. "Ein zweiter Apparat, mit dem in einer von der soeben beschriebenen etwas abweichenden Weise sowohl Umfangsbestimmungen der Aufmerksamkeit durch momentane Apperceptionsversuche, *wie solche des Gesamtbewusstseins* . . . ausgeführt werden können," etc. (Italics are mine.)

¹ *Psych. Stud.*, III, 387.

² *Ibid.*, 390. (Italics are mine.)

³ *Psych. Stud.*, V, 69.

⁴ *Zeits. f. Psych.*, LIII, 1909, 97-102.

dass beide Reihen nicht annähernd parallel verlaufen,—was nach der Verschiedenheit der psychologischen Bedingungen beider Methoden auch zu erwarten war, wodurch aber auch die Möglichkeit einer objektiven Wiedergabe des Klarheitsreliefs nach beiden Methoden in Frage gestellt wird.”¹ I confess, too, that I do not see why, if Wirth objects to the assumption that his reaction-experiments were meant to be “eine Methode zur Messung der Klarheitsgrade,”² he should compare their results with those of his *Schwellenmethode*. And why does he speak of an “Übereinstimmung der beiderseitigen Aufmerksamkeitsmasse?”³ Surely, such language is both inconsistent and misleading.

Aside from these more technical issues, there remain certain important points concerning which a real disagreement seems to exist between Wirth and myself. In the first place, I cannot follow Wirth in drawing a sharp distinction between “the activity of attention as the most important subjective condition of a certain formation of the clearness relieve” and “the actually attained degree of clearness” (§ 3).⁴ I myself never experience anything like an ‘activity,’ which might be supposed to be under voluntary control, nor have I ever met with it in the introspections of my observers; and the psychologists who occasionally mention it in experimental contexts, do not agree as to its nature. Hence the meaning of the phrase is not as unequivocal as Wirth assumes it to be.⁵ For my own part, I must continue to identify the ‘activity of attention’ with the attained degree of clearness.

Another point upon which I disagree with Wirth concerns the general nature of the attentive consciousness, especially in the tachistoscopic experiments. I happen to have taken part lately in over one thousand monocular tachistoscopic observations on liminal and slightly supraliminal brightness-

¹ *Ibid.*, 99.

² *Psych. Stud.*, V, 69.

³ *Psych. Stud.*, IV, 156.

⁴ *Psych. Stud.*, V, 52.

⁵ Perhaps Wirth’s distinction is meant to be identical with Wundt’s distinction between *Aufmerksamkeit* and *Apperception*, as it is presented in the *Physiol. Psych.*, III, especially 341: “Nach allem diesem sind Aufmerksamkeit und Apperception Ausdrücke für einen und denselben psychologischen Thatbestand. Den ersten dieser Ausdrücke wählen wir vorzugsweise, um die subjective Seite dieses Thatbestandes, die begleitenden Gefühle und Empfindungen, zu bezeichnen; mit dem zweiten deuten wir hauptsächlich die objectiven Erfolge, die Veränderungen in der Beschaffenheit der Bewusstseinsinhalte an.” Wirth’s “activity of attention” would then be identical with Wundt’s “Thätigkeitsgefühl.” But it is well known that this side of Wundt’s apperception theory has been sharply criticised, and has found little acceptance.

differences which, placed about 12cm to the left of a constant fixation point, were exposed for one hundredth of a second and judged under three different distributions of attention. The conditions were very similar to Wirth's, although, of course, they were far less complicated. In these observations, my attention was for the moment raised to the very highest pitch of concentration, while the background was so obscure that I could not possibly analyze it. I have found certain observers who agree with me in experiencing, under these conditions, a marked difference between maximal and minimal clearness-levels, while others distinguish simultaneous processes of intermediate clearness-degrees. This difference, which is borne out by the results of a large number of systematic experiments on degrees of clearness,¹ has led me to assume two different types of the attentive consciousness, the 'dual-division' and the 'multi-level' formation. Evidently Wirth, and probably also Wundt, belong to the latter type. Whether Wirth's results would have been different if he had had observers of the first type, it is impossible to say. But it still remains a disadvantage of his experimental work that in all of the final series he was the sole observer (§ 10). The same objection is implied in Grünbaum's review, although he refrains from making it an explicit criticism.² It is true that Wirth made preliminary experiments with several less skillful observers; and he regards the results as utilizable, because they show a rough agreement with some of his own observations obtained at an early stage of practice. Nevertheless, these other observers made binocular, and not monocular observations upon a plane, and not upon a funnel-shaped field of vision; so that the experimental conditions in their case were quite different from (much simpler and easier than) those under which Wirth himself later completed the final series. I might add that Grünbaum also considers Wirth's experimental conditions as too complicated and artificial, and the requirements made upon the observer's attention and introspective self-control as too difficult (if not even self-contradictory), to guarantee a successful solution of the problem.

Finally, Wirth objects to my using collectively the results of his *Schwellenmethode* (§ 11). I combined into a single frequency curve all the numerical clearness values given in his six schemata, each schema representing a different distribution of attention. The reason for his objection is that some of the schemata were obtained at an earlier, some at a later stage of

¹ The Measurement of Attention, AM. JOUR. OF PSYCH., XX, 1909, 473-529.

² *Op. cit.*, 98.

practice in observation. Nevertheless, in his own final conclusions, Wirth neglects this practice-effect altogether, and directly compares with one another the results expressed in the various schemata. Besides, I had, for my own satisfaction, constructed individual frequency curves for all the different distributions of attention, and had found great similarity among them; so that for my particular purpose I saw no further necessity of separate treatment. However, even if the different stages of practice had influenced the results, I might justly have followed Wirth's example in his application of the principle of analogy, and might have assumed that longer practice would probably eliminate large irregularities in the frequency distribution.

In conclusion, I still maintain that Wirth has failed to solve his problem, not because, as he tries to show, I had been mistaken in its nature, but because, as I have said before, his numerical clearness values were "obscured or invalidated by complicating factors," and because "the restricting conditions of his experimental arrangement" served only to enhance "the impossibility of overcoming difficulties of observation" whose existence he himself has frequently admitted.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XII. THE SOURCES OF THE AFFECTIVE REACTION TO FALLACIES

By ANNA H. TAYLOR and M. F. WASHBURN

About a hundred young women students, mostly college Juniors, were asked to introspect the impression produced upon them by a set of logical fallacies. In the original plan six of these faulty arguments were used, the design being to have two specimens of each of the three types of formal fallacy: illicit major, illicit minor, and undistributed middle; and to construct one of each pair with a true conclusion, the other with a false conclusion. In practice, the syllogism used to represent the fallacy of illicit minor with a true conclusion proved unsatisfactory, being so obviously wrong as not to produce the impression of an argument at all, and it was therefore discarded. The observers were asked in the first instance to record whether the arguments were agreeable or disagreeable, and then to report any further considerations that occurred to them. The great majority of them had had no training in formal logic. Nineteen had pursued a course in argumentation, and seven had studied logic in preparatory schools. Fifty-four, however, had had a course in introductory philosophy, in which the syllogism had been briefly explained.

The following were the faulty syllogisms used:

1. (Undistributed Middle: true conclusion.) "All trees are vegetables; all oaks are vegetables, therefore all oaks are trees."
2. (Same: false conclusion.) "Virtuous people always make profitable use of their time; day laborers make profitable use of their time, therefore day laborers are virtuous."
3. (Illicit Minor: false conclusion.) "Only criminals should be put under restraint; for all criminals are dangerous to society, and all persons who are dangerous to society should be restrained."
4. (Illicit Major: true conclusion.) "Church property is not taxed; for it is not private property, and all private property is taxed."
5. (Same: false conclusion.) "Mathematical study improves the reasoning powers; but as the study of logic is not mathematical study, we may infer that it does not improve the reasoning powers."

The papers that were handed in proved to contain material of much interest, but material with which it has been by no means easy to deal. As any one who has undertaken a similar task knows, analyzing and classifying a large mass of introspective results involves the danger of falsifying them. In the desire to bring order out of chaos, statements are brought together under the same heading that represent really different mental processes; and in general much of the value of the introspections is lost by the cutting and drying process to which they are subjected. A great amount of the most careful study has been devoted to the reports of our observers in the

present instance; they have been gone over again and again, and as a result we think we have gathered some information about the sources through which a mistake in reasoning produces an unpleasant effect upon the mind. These sources, as they appear from the introspections of our observers, we shall now discuss one by one.

(a) The *content* of the ideas contained in the syllogism. By this is meant that in a certain number of cases, the first affective reaction of the observer was to the agreeable or disagreeable character of one of the terms of the argument, or some directly suggested idea; and in other cases this was one of the sources of pleasantness or unpleasantness. For example, in (3), the idea of criminals, or of the insane, was unpleasant; in (2), the idea of day-laborers and in (5), the idea of mathematics were disagreeable. The number of observers reporting this as an important source of their affective reaction was for (2), eight; for (3), four; for (4), one, and for (5), five. Evidently such a source of pleasantness or unpleasantness as this is not in any way characteristic of the reasoning processes as such. The ideas of mathematics or of criminals would have been just as unpleasant if they had been suggested outside of any argumentative context, for instance, in a series of disconnected words. In the case of the first argument, a special instance of affective reaction determined by the content of the idea was the unpleasantness experienced by many of the observers on account of the incongruous images suggested by 'tree' and 'vegetable.' Eight persons gave this as the only reason for finding the syllogism unpleasant, and two of these said that as soon as 'vegetable' was understood in the scientific sense the argument became pleasant; thus evidently wholly overlooking the fallacy. Fifteen others found the incongruity one among various reasons for the unpleasantness of the syllogism. Incongruity, as a source of unpleasantness, is evidently 'relational' in character; it involves what is commonly known as thought rather more than do the other instances of the content of ideas as affective source, where nothing more than the mere reproduction of an image is necessary.

(b) The *truth or falsity* of the statements. In the case of the second argument twelve persons reported that they found it unpleasant merely because they denied the truth either of the conclusion, or of the conclusion and one of the premises. Eight persons gave the falsity of the conclusion in (3) as their only reason for disliking the syllogism, and three mentioned it as one among other reasons. Three observers reported the falsity of the conclusion, in (5), as the only source of unpleasantness, and five gave it as one source. What mental processes are involved in recognizing that a statement is false? Our data throw light on this problem in a few cases only. The conclusion of (2), that day-laborers are virtuous, aroused in those observers who analyzed their consciousness of its falsity a sense of the incongruity of the subject and predicate; several reported that they had a mental picture of a gang of workmen, dirty, quarrelsome, and disorderly. In other cases it is probable that the consciousness of incongruity was present without involving any images. The conclusion of (3), that only criminals should be put under restraint, instantly suggested to most of the observers who were disturbed by its falsity the idea of insane persons at large; in three cases the idea of a particular insane person. Here the hitch came between the word 'only' in the conclusion and the thought of the insane, and the feeling not merely of incongruity but of incompatibility or contradiction was aroused. Into the more ultimate nature of these "feelings" our data do not allow us to go. In the case of (5), those observers who were disturbed by the falsity of the conclusion that logic does not improve

the reasoning powers seemed, as nearly as we could judge, merely to be conscious of the contradiction between this statement and statements to the contrary which they had previously met with; that is, no images were called up, and very likely only verbal formulas regarding the value of logic were suggested, if anything more than a bare 'feeling of dissent' was present. In so far as we get any information from our results, the consciousness of the falsity of a statement may involve (1) a vague feeling of dissent or negation, (2) a feeling of incongruity or incompatibility between the subject and predicate of the statement; (3) a feeling of incongruity or incompatibility between some idea contained in the statement and other ideas not contained in it but suggested by it. Whatever its nature, evidently the consciousness of the falsity of one of the statements in an argument is something aside from the process of reasoning involved in the argument itself; the statement would be unpleasant on account of its untruth even if it stood alone and unconnected with the other propositions of a syllogism, just as the content of an idea might be disagreeable although the idea was suggested in isolation.

(c) A sense that something *definite* has been *omitted*. In the first syllogism, each one of the three propositions is true. Six of the observers recorded that they experienced unpleasantness from this argument because they thought at once of many other vegetables besides those mentioned in the premises. Here for the first time we have a source of unpleasantness that is truly logical. Only a part of the 'middle term' is referred to in either premise, and the reasoning process, which ought to pass smoothly from minor to middle and from middle to major term, is broken and interrupted by the occurrence to the mind of other ideas, such as those of cabbages and carrots, which are as naturally suggested by the middle term 'vegetables' as is the major term 'trees.' With the occurrence of these other ideas comes a sense that they should have been taken account of in the argument, and a consequent unpleasant affective tone. Four observers found the chief source of unpleasantness in the fourth argument to lie in the thought of other kinds of property being taxable besides private property. Here the discomfort seems to arise from the fact that the thought process, which should pass smoothly along the course: 'church property—other than private property—not taxed,' is led off from the middle term 'other than private property' to the thought of cases of public property that are taxed. None of the observers, however, reported that they thought of specific instances of public property being taxed; the 'sense of something omitted' was not as definite as in the case of the first syllogism. In (3), the idea of insane people presented itself with more or less definiteness as needing to be taken account of to render the argument logical (not merely to make the conclusion true) in the minds of nineteen of the observers, if we may judge by the fact that this number of them said the word 'only' should be changed to 'all.'

(d) Closely connected with the source of unpleasantness just mentioned is one which may indeed be the same experience in a different stage of development: the sense of a definite *lack of equivalence* between the terms. For example, in the case of (1), five of the observers said the argument was unpleasant because "two sub-classes of the same class are not necessarily identical," or words to that effect. Seven others found it disagreeable because it involved an attempt to apply the mathematical axiom that things equal to the same thing are equal to each other, where such an application could not be made. In the case of (2), eight persons said that one quality common to two classes did not make them identical, and four put the same idea from

what the logician would call the point of view of extension, by saying that two parts of the same class are not necessarily identical. In these instances the observers' state of mind seemed to be not so much the consciousness of certain ideas that should have been but were not included in the argument, like the 'carrots and cabbages' in (1) or the insane people in (3), as a more vague and abstract mental process for which no more fitting descriptive term than 'consciousness of inequality' presents itself. Such a mental process must be of frequent occurrence in purely mathematical reasoning. It probably varies from a practically unanalyzable 'relational' process, through a more complex process associated with some vague ideas of the omitted factors, the other parts of the class which have not been taken into account, and thus by imperceptible gradations may pass into a process involving definite thought of the neglected ideas, such as we have discussed under (c). It is to be noted that the mere 'consciousness of inequality' would not of itself be a source of unpleasantness; the unpleasantness must arise from the fact that the terms *ought not* to be unequal. The thought of two sub-classes as not being identical with each other would not be disagreeable unless the argument demanded their equality. Is it not probable that the unpleasantness in this case arises ultimately from the same source as it did in (c); that is, from the confusion and division of attention that result when instead of passing smoothly from one term to the next, attention is 'led aside' to consider the omitted factors? In the instances we are considering, the omitted factors are not definitely thought of as they are in the cases under (c), but even although they are represented only by an unanalyzable relational process, the affective tone which would accompany the clear and developed thought of them may be transferred to the relational 'consciousness of inequality.'

(e) Under this head we may consider the cases where unpleasantness arose because of a quite vague and *indefinite sense* of *something wrong* with the argument. The observer cannot or at least does not state that anything is wrong with a particular part of the reasoning; she does not 'place' the wrongness; it is only vaguely felt. "Something is wrong with the 'therefore,'" said two observers in the case of syllogism (1). Two persons complained that (5) was 'unconvincing' and therefore unpleasant. Four gave 'incompleteness' without further specification as the source of unpleasantness in (1). It is, of course, impossible to be sure, with our untrained observers, that the sense of incompleteness was in every case perfectly vague; the omitted factors might have been thought of with some definiteness although the observer did not take the trouble to report the fact. But it seems probable that a sense of something lacking did sometimes accompany the reading of the arguments without being attached to anything definite. The term most frequently used to describe the vague sense of something wrong was 'confusion.' Five persons named this as the sole source of unpleasantness in the first syllogism; three in the second, four in the third, five in the fourth, and two in the fifth. 'Confusion' would seem to be one degree vaguer than a sense of 'something omitted.' One might enumerate the logical sources of the unpleasantness of a fallacy in the following order, beginning with the most indefinite, the passage from each stage to the next being brought about by better attention and analysis: general sense of confusion, sense of something omitted, sense of a lack of equivalence between two of the terms, clear idea of the omitted factors. All of these processes are unpleasant for what is ultimately the same reason: a tendency to division of the attention instead of allowing it to pass smoothly from one term to the next.

(f) It will not be inappropriate to consider under a special heading the reports of certain observers who were distinguished from the rest through taking what we may call a *subjective* attitude towards the arguments, or perhaps 'personal attitude' would describe it better. In most of these cases there was probably recognition of the fallacy, in some sort of terms, but the unpleasantness attached either to the thought of the observer herself as being the subject of an attempt to deceive, or to the imaginary propounder of the argument. In the case of (1), three persons described their attitude as one of 'irritation' at the maker of the argument; one said she felt 'scorn,' another confessed to a 'pugilistic impulse,' and still another declared the author of the syllogism to be "beyond the reach of argument." Three, on the other hand, felt wounded self-esteem, a 'sense of being deceived.' These experiences were mentioned in the case of the other arguments also. They show how close the connection may be between intellectual processes and the fighting instinct.

(g) Three observers gave their general dislike of formal arguments as the chief reason for finding the syllogisms unpleasant.

We must also take into account those observers who found the arguments either wholly or partly *pleasant*, and those who experienced no reaction at all. The percentage of observers finding the syllogisms wholly agreeable was for (1), seven; for (2) twelve; for (3), ten; for (4), twelve, and for (5), nine. The percentage of observers finding the arguments partly pleasant and partly unpleasant was, for (1), eleven; for (2), three; for (3) six; for (4), five, and for (5), four. The percentage of those finding the arguments neither agreeable nor disagreeable was for (1), ten; for (2), fourteen; for (3), eighteen; for (4), twenty, and for (5), seventeen.

What were the sources of pleasantness? In a few scattered instances they were found in the content of the ideas suggested, as when one observer declared that she found (2) pleasant because it was agreeable to think of virtuous people making profitable use of their time. But the principal sources were (a) thinking the syllogism correct, which was done by ten per cent. of the observers in the case of (4), by two per cent. in the case of (1), and by two per cent. in the case of (3); (b) amusement, which was felt by from one to six per cent. of the observers in the case of every argument, and (c) the enjoyment of tracing and correcting the fallacy, which from two to four per cent. reported with all the arguments. The comparatively large number of persons who thought the fourth syllogism correct may perhaps be accounted for by the unfamiliar character of its subject-matter, which very likely obscured the logical processes involved. No special discussion of these sources of pleasantness seems justified by the data at our command. And if, in general our discussion and analysis of the experiences of our observers appears to be far from thorough, our excuse must lie in the fact that their introspective records were not full enough to warrant us in interpreting the results further. If the observers had been trained in introspection we should have gained much, but we should have also lost something, in that our subjects would have been alike less numerous and less naïve.

XIII. SOME TESTS BY THE ASSOCIATION REACTION METHOD OF MENTAL DIAGNOSIS

By HAZEL M. LEACH and M. F. WASHBURN

In the experiments whose results are discussed below we followed the method of Yerkes and Berry (this *Journal*, Vol. 20, page 22), with some slight modifications; our design being to test the method on as many observers as possible. Each observer was first given a practice series of twenty-five words, being told that on hearing a word she was to speak as quickly as possible the first associated word that occurred to her. The reaction times were taken with a stop-watch. The observer was then directed to go to another part of the laboratory and open one of two boxes that she would find there. On her return she was told that she must try by every possible means to keep the experimenter from learning into which box she had looked. Her reactions to a series of sixty words were then taken. In this series there were ten words referring to the contents of one of the boxes, and ten referring to those of the other box. These relevant words were scattered in among the others, but three or four words referring to the same object were given in immediate succession to increase their disturbing effect upon the observer. Twenty-six persons were tested, and all but one of these were observers in two experiments each, with different pairs of boxes. In one experiment, the objects in the boxes were a bottle of red ink and a watch; in the other, they were a toy snake and a rag doll. Later a pack of cards was substituted for the doll. The following were the stimulus words:

Series I. Ink bottle—watch.

- | | | |
|---------------|---------------|------------------|
| 1. View. | 21. Door. | 41. Pond. |
| 2. Bird. | 22. Window. | 42. Drama. |
| 3. Lily. | 23. Evening. | 43. Church. |
| 4. Satin. | 24. Sugar. | 44. Bottle. |
| 5. Stitch. | 25. Sharp. | 45. Eraser. |
| 6. Position. | 26. Blotter. | 46. Cork. |
| 7. Walk. | 27. Pen. | 47. Indian. |
| 8. Spring.— | 28. Glass. | 48. Remark. |
| 9. Tick.— | 29. Red. | 49. Star. |
| 10. Second.— | 30. Smile. | 50. Clay. |
| 11. Hands.— | 31. Heroine. | 51. Doll. |
| 12. Good. | 32. Anthem. | 52. Fish. |
| 13. Indoors. | 33. Columbus. | 53. Orange. |
| 14. Nobody. | 34. Shop. | 54. Gold.— |
| 15. Sleep. | 35. Lamp. | 55. Fob.— |
| 16. Cow. | 36. Time.— | 56. Mainspring.— |
| 17. Carter's. | 37. Elgin.— | 57. Nails. |
| 18. Fluid. | 38. Hour.— | 58. Floor. |
| 19. Write. | 39. Boy. | 59. Box. |
| 20. Meat. | 40. Castle. | 60. Drink. |

It will be seen that numbers 8, 9, 10, 11, 36, 37, 38, 54, 55, and 56 are the relevant words for 'watch'; while numbers 17, 18, 19, 26, 27, 28, 29, 44, 45, and 46 are the relevant words for 'red ink bottle.'

Series II. Snake—Doll (Cards).

1. House.	21. Finger (Dia-	41. Rattle.
2. Earthquake.	mond).	42. Train.
3. Sky.	22. Question (Joke).	43. Liberty.
4. Telepathy.	23. Net.	44. Sun.
5. Clue.	24. Horse.	45. Washington.
6. Tree.	25. Royal.	46. Law.
7. Radius.	26. Companion.	47. Pencil.
8. Boston.	27. Word.	48. Teddy Bear
9. Gray.	28. Disgrace.	(Play).
10. Crawl.	29. Nothing.	49. Stuffed (Spade).
11. Slimy.	30. Baby (Club).	50. Blue (Heart).
12. Bite.	31. Dress (Deal).	51. Lips (Question).
13. Study.	32. Calico (Card).	52. President.
14. Morning.	33. Fiction.	53. Salt.
15. Room.	34. Suffrage.	54. Tennis.
16. Comfort.	35. Shock.	55. Friendship.
17. Rain.	36. Home.	56. Ticket.
18. Apple.	37. Picture.	57. Adder.
19. Face (Bridge).	38. Wave.	58. Wriggle.
20. Red (King).	39. Coil.	59. Fang.
	40. Poison.	60. Servant.

The words in parentheses were used only in the experiments where the cards were substituted for the doll. Numbers 9, 10, 11, 12, 39, 40, 41, 57, 58, and 59 were relevant to 'snake'; numbers 19, 20, 21, 30, 31, 32, 48, 49, 50, and 51 to 'doll'; numbers 19, 20, 21, 22, 30, 31, 32, 48, 49, and 50 to 'cards.'

One of the authors of the paper, H. M. L., acted as experimenter throughout. The results did not come into the hands of the other author until after the entire series of experiments was finished and H. M. L. had recorded her judgments as to which box the observer had investigated in every case. H. M. L.'s decision was based partly on the average reaction times for the words referring to one object, as compared with the average reaction time for words referring to the other object; partly on the character of the associations. In one single experiment only, that is, one of the two series given to one of the observers, was this judgment incorrect. This means one error out of fifty-three experiments. *In all the experiments but four, the average reaction time for words referring to the object actually looked at was longer than that for the words referring to the other object.* One of these four cases was the case where H. M. L.'s judgment was incorrect; in the other three she judged correctly from the nature of the associations. It was noteworthy that the *shortest two average reaction times on the entire list were those for words referring to the object seen*, forming two of the four exceptions to the rule that the average reaction time to such words is long. H. M. L. concludes from this that in rare cases the dangerous words are reacted to with abnormal quickness. In both the instances here noted, the observers made no effort to prevent the significant words from suggesting the object seen, and the result was that such suggestions came more rapidly than the rest. *Yet in both these cases the longest single reaction time was that for a relevant word.* In all but six of the experiments, the mean variation for words referring to the object seen was longer than that for words referring to the other object; thus the reaction times

to relevant words seem usually, though not invariably, more irregular as well as longer than those for words referring to the other object.

When the results were turned over to M. F. W., it occurred to her that it would be interesting to see how far correct judgments could be based on the nature of the associations alone, leaving the times out of account altogether. She accordingly went over all the associations, with the times concealed, and tried to discover in each experiment which of the two objects had been seen by the observer. The aim of this was not so much to test the practicability of ignoring reaction times,—for there would be no advantage in leaving them out of account,—as to study the nature of the considerations that enable one to base a judgment on the character of the associations, a factor that always has to be taken into account. The total number of experimental results examined was fifty-two; in thirty-four of these a correct judgment was reached; in sixteen, a wrong judgment, and in two cases it was impossible to decide. The number of correct judgments that might be accounted for by chance was twenty-five. Evidently reaction-time alone is a much safer guide than the character of the associations alone.

What, now, are the peculiarities of the associations made which lead one to infer that a particular object has or has not been seen? After examining the associated words in each experiment, M. F. W. recorded her judgment and the reasons for it; and a careful study of these reasons seems to show that the types of self-betrayal fall into two classes according as the observer does or does not try to avoid significant associations. When there is *no effort to avoid*, we have, first, the cases where *a relevant stimulus word suggests a word that would not naturally occur to a person who had not seen the object*. This may be called the most naïve type; it would be the natural method of self-betrayal if there were no effort to conceal. It proved to be rare in these experiments, and to be an uncertain indication. Only one correct judgment was based upon it, where the word 'red' suggested 'ink,' as it would not have been very likely to do unless the red ink bottle had been seen. On the other hand, in two cases an incorrect judgment was reached on the same basis; once the object was wrongly judged to have been 'snake' because 'coil' suggested 'snake,' and another time it was judged to have been the ink bottle because 'fluid' suggested 'ink.' Needless to say, the judgments in both these cases were merely tentative; the unsatisfactory nature of the evidence was recognized, and it was influential only because no other basis for judgment presented itself. More convincing were the cases where *a relevant word gave an association different from that which would have occurred had the object to which the stimulus word referred been seen*. Thus in two cases correct judgments were reached because the stimulus word 'write' was perceived as 'right,' which would hardly have been the case had the ink bottle been seen. This form of self-betrayal is classed with the preceding because in neither case need there be any effort to conceal on the observer's part. Under this same head should come the cases where *the significant associations are given with abnormal frequency*. Thus in one series where the watch had been seen, the only reaction words given were 'watch' and 'time,' repeated again and again; in another case where the cards had been seen 'cards' was given five times as a reaction word, and in still another, where the object was the ink bottle, 'ink' was given six times. On the other hand this kind of evidence twice led to a wrong conclusion, 'snake' being given seven times and again five times when it was not the object seen. If this form of supposed self-betrayal could be relied upon, it would be because it indi-

cated a kind of recklessness in the observer's mind; a more disturbed condition than is suggested by the single occurrence of a significant association. The latter would happen to an observer who had no thought of concealment at all; the former, one might suppose, would be more likely to occur with one who had made up his mind that the best way to conceal would be to say whatever came into his head. However, the fact that such repetitions did occur with reference to an object that had not been seen at all, shows that it is unsafe to argue in this manner.

Over against these we may place the cases giving evidence of *forced avoidance of significant associations*. In eight instances a correct judgment was reached partly or wholly on the ground that not one of the relevant words suggested anything connected with the object to which it was intended to refer; that is, the avoidance of significant associations was *complete*. It was concluded that such avoidance must have been intentional. But there were two cases in which a wrong judgment was reached on this basis; not a single significant association occurred, and yet there was no intention to avoid, since the object to which the relevant words referred had not been seen at all. Then there were three correct judgments made on the basis of *avoidance of a relevant association under circumstances that would naturally have suggested it*: (1) after 'king' had suggested 'card,' 'diamond' suggested 'bracelet,' whereas it would in the mind of a person whose associations were normal have continued the 'card' train of ideas; (2) after 'tick' had suggested 'stop-watch,' 'second' suggested 'street;' (3) after 'fob' had suggested 'watch fob,' 'mainspring' suggested 'Main Street.' Here again we have to offset these instances with a case of failure by the same method, but the mistake occurred where the application of the principle is by no means so clear as it was in the series just mentioned. After 'spade' suggested 'hearts,' 'heart' suggested 'feelings;' it was wrongly concluded that the pack of cards had been seen, but the passage to a different line of associations here might have been due to the fact that the stimulus word 'heart' was a repetition of the last reaction word, so that the observer was not unnaturally given an impulse to break away from that set of ideas in a new direction. The effort to avoid significant associations shows itself also, as has often been pointed out, in the occurrence of *far-fetched* associations. The following is a list of the associations of this character upon which correct judgments were based: 'calico—variegated colors,' 'fang—boy,' 'second—how many in an hour,' 'time—big idea,' 'deal—what kind,' 'Elgin—make,' 'mainspring—mechanism,' 'tick—glasses,' 'second—tie,' 'time—guess,' 'slimy—dessert,' 'heart—chicken,' 'Elgin—Europe,' 'mainspring—lake,' 'tick—year,' 'mainspring—time,' 'hour—National Park.' In some series two or more of these 'forced' associations were found; altogether, eleven correct judgments were based wholly or partly upon them. They may not, as reported above, seem particularly far-fetched in every case, but they were always 'peculiar,' as compared with the other associations in the series. Three erroneous conclusions were formed on account of the occurrence of associations, at least as forced as any of those above, viz.: 'fang—fishes,' 'deal—page,' 'card—hardness' (the last two occurred in the same series), 'wriggle—little girl of that name.' Again, the effort to avoid is betrayed by the occurrence of *sound or rhyme associations*. These were found in the case of two observers only, and a correct conclusion was drawn in each case. One gave the following associations: 'tick—click,' 'time—last time,' 'hour—glower,' 'gold—golden;' the object seen being the gold watch. The other observer, who had seen the snake, gave:

'crawl—sound of word,' 'wriggle—rig,' 'fang—hang.' Still another indication of an effort to avoid significant associations lies in *repeating the previous reaction word*. Thrice this betrayed the object seen: once when 'wriggle' and 'fang' both suggested 'lizard,' once when 'medicine' was suggested by both 'bottle' and 'cork,' and once when 'poison' and 'rattle' both suggested 'antidote.' In one series the observer took refuge in the word 'animal,' to conceal the fact that she had seen the snake; 'animal' was given in response to 'gray,' 'bite,' 'rattle,' and 'adder.' Once the *stimulus word itself*, 'hour,' was repeated, leading to the correct conclusion that the watch was the object seen. Finally, suspicion was aroused in one instance because the reaction word seemed to show a *reversion back to the preceding stimulus word*: 'tick' suggested 'watch' and the next word, 'second,' suggested 'ticking,' but the conclusion that the observer had seen the watch proved to be untrue.

It is thus evident how uncertain are the inferences based on the character of the associations alone. Judging from these results, the most trustworthy evidence is furnished by a misunderstanding of the stimulus word in a way that would be practically impossible for a person with the object to which it referred in mind, as when 'write,' which referred to the ink bottle, was heard 'right' and suggested 'wrong;' and by the occurrence of associations based on the sound of the word. Yet if 'write' had suggested 'wrong' only after an abnormally long interval, one might correctly infer that this was a forced association, so that it would be necessary to take account of the reaction time here also; while it may have been mere chance that the rhyming associations furnished the basis of none but correct judgments in these experiments of ours. There seems to be no *a priori* reason why undue emphasis on the sound of a word should always mean a desire to avoid associations connected with its meaning.

The best single criterion on which to base a decision as to the object seen by the observer in these experiments was the *longest single reaction time*. If, in each series, the longest reaction time was selected, and was found to be that of the reaction to a relevant word, this word proved in every case but two out of the fifty-three to refer to the object seen by the observer. If the longest time belonged to the reaction to an irrelevant word, the next longest was taken, and so on until one that was connected with a relevant word was reached. *The longest single reaction time to a relevant word proved, in every series but two, to be that of a reaction to a word referring to the object seen.* This record is better than that of the average reaction times; it will be remembered that there were four exceptions to the rule that these are longest for words connected with the object looked at. The exceptions to the rule of the longest single reaction time were among these four. In one of the exceptional series, the object seen was the pack of cards; the longest reaction time was for the association 'earthquake—Messina,' earthquake being an irrelevant word. Next longest was that for 'slimy—ooze,' 'slimy' referring to the snake, which had not been seen. Both of these delays might be accounted for by the emotional significance of the stimulus words. In the case of the other exception, the object looked at was the watch, but the longest single reaction time was that for 'Carter's—people,' in which the stimulus word referred to the ink-bottle. Here the delay was doubtless owing to the fact that a person who had not seen the ink-bottle would be somewhat puzzled by the unusual stimulus word 'Carter's.' There were eight cases where the longest single reaction time was that of the association with an irrelevant word, but where the longest

time of reaction to a relevant word was that of association with a word referring to the object seen. The irrelevant stimulus words that gave abnormally long reactions were as follows: (1) 'Position—too indefinite;' here the abstract character of the stimulus word seems to have caused the delay. (2) 'Clue—detective;' the next longest r. t. was that for 'bite—scratch.' These stimulus words probably had some affective accompaniment that caused the delay. (3) 'Remark—word;' here there is no discoverable reason for the long r. t., and the same is true of (4), 'house—brown,' and (5), 'walk—parachute.' If 'walk' had been a relevant word, the far-fetched character of this association together with the fact that its time was so long, would have led to the positive conviction that the object connected with 'walk' had been seen. Unfortunately the observer's introspection on this association was not recorded. In the case of (6), 'telepathy—man,' the unusual character of the stimulus word may again have been, at least in part, responsible for the delay; with (7), 'disgrace—prison,' affective influences undoubtedly played a part. In case (8), the longest three reaction times were to irrelevant words: 'comfort—vague idea;' 'servant—play;' 'disgrace—sin.' The last two involved affective elements (the observer had recently seen "The Servant in the House"); the first gave a slow reaction probably because of its abstract character. In general it may be said that when a word referring to an object not seen gives an abnormally long reaction time this is because it (a) is of a disturbing affective character in general, like 'disgrace;' (b) refers to some emotional complex, peculiar to the observer, or (c) is abstract or unusual in character. It would seem that the rule that the longest single reaction time to a relevant word is to a word associated with the object seen may be safely followed except where some word referring to the other object comes under one of the three classes just mentioned. One could never be sure of selecting all the relevant words in a series so that none should be connected with an emotional complex peculiar to the observer, but it would be possible to avoid using, as relevant words, those of general emotional import, or those which were unusual or abstract.

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- Schriften zur angewandten Seelenkunde*, herausgegeben von SIGMUND FREUD. Franz Deuticke, Leipzig und Wien.
- Selected Papers on Hysteria and Other Psychoneuroses*, by SIGMUND FREUD. Authorized Translation, by A. A. BRILLE. The Journal of Nervous and Mental Disease Publishing Co., New York, 1909. 200 p. (Nervous and Mental Disease Monograph Series No. 4.)

The first six of these pamphlets on applied psychology, edited by Freud himself and written or to be written by various pupils of his, are designed to bring together into one convenient series the characteristic view-points of what is, to the mind of the present writer, by far the most interesting and original line of development which modern psychology has had since the experimental movement began with Wundt. True, the Freud school have based their conclusions chiefly on the study of more or less abnormal cases; but the psycho-analytic method is only a further development of Wundt's association reactions; while the abnormal features by the Freudists, as indeed now by most psychiatrists, are regarded as modifications of the normal, some traits of which are magnified, others suppressed, so that we have simply to look on and see nature experiment. Wundt has always allowed but the smallest range to unconscious psychic processes. For him it is but a very little way from consciousness down to merely physiological processes. This error has been fatal to Wundt's influence among those who deal with every class of mental defectiveness. No psychology can abide that does not stand this perhaps most important of practical tests; and here, by unanimous and reiterated consent of those in or near the Freud camp, Wundt has already signally failed. Those who study mental alienation are drifting farther from him.

In a different way and for different reasons, Kraepelin, too, is being left behind. This writer started from the Wundtian basis and has done signal service for the world by his breaking up of the rigid, old, and the development of new, classifications. He has, however, never succeeded in reaching any very stable equilibrium, as witness the

essential changes of view-point in his successive editions. Great as his contribution has been, there can be no doubt whatever that he is being slowly left behind by those more interested in minute and careful observation, who are ready to follow facts wherever they lead and are unencumbered by schematizations which, in the Kraepelin camp, have become altogether too cumbersome. Those who have followed the Freudian literature know with what infinite patience and detail the clinical cases are followed up as the investigator penetrates to layer below layer of the patient's soul.

Freud himself is a man of wide reading and of keen literary insight and taste. His pages abound in allusions to literary masterpieces from the Greek drama down to the contemporary novels; but allusions to these he uses as entirely subordinate to his main purpose. In Vienna he has had scant recognition, being, although on in the fifties, only "extraordinary" professor; and he has been largely absorbed by his duties as a medical practitioner. Owing to the large place which he assigns to sex in the development of psychoneuroses, he has been misunderstood and for a time suffered socially. This, however, is happily past and he is now by every token likely to receive full recognition. It was an experience never to be forgotten by those who shared it to listen to the daily lectures for a week in September that he gave at the Clark conference. He spoke in German and without notes, and in a voice of so little power that his hearers drew their chairs in a semi-circle about him. But never in the writer's experience have a group of advanced scholars, many of whom have achieved great eminence in this country, listened with greater interest to the words of a great teacher. His expositions were masterpieces of simplicity, and it is hoped and believed that the lucidity of his expositions, supplemented as they were by a number of private conferences and one most successful demonstration of his method in a private clinic, will lead to the recognition he deserves in this country. One of his leading disciples, Dr. Jung, of Zurich, accompanied him and also gave a series of lectures in German. Fortunately, too, we now have the above first translation into English of a few of his selected papers. Freud regards his as yet untranslated work on the interpretation of dreams as the key to his system. This, especially the enlarged second edition upon which so much time and labor have been expended, is the best introduction to his work, although it must be admitted that it is hard reading, owing to its technical terms. This, with his other books on wit and on the psychology of daily life, show how his views apply to all the occurrences of human life, waking or sleeping, in health as well as in disease.

Most of his patients have been women and, owing to his fame, many of his cures have been cases of long standing, who have passed through courses of treatment by other physicians. Perhaps the chief common criticism directed against him is that he magnifies the importance of sex in both health and disease. His own claim is that the immense rôle which this function plays in nearly every form of human ill with nervous complications has hitherto been for various reasons vastly underrated, that its manifestations have been everywhere repressed, and that its influence is profoundly felt at every stage of life beginning with infancy. In this respect and in the thoroughness of his analysis and in his disuse of hypnotism, he differs from his own great teacher, Charcot, and from his earlier and older associate, Breuer, as well as from Janet. To our own thinking, his system so far as it is at present developed, lacks one essential thing, and that is adequate recognition of psychic evolution and the influence of past stages of the soul's development upon its present forms of aberration.

In his last book, "Les Névroses," Janet in the few concluding pages has taken the epoch-making step of acknowledging in general terms how a complete understanding of such cases requires us to understand more than is at present known of the early stages of the development of the soul.

Le Doule, par PAUL SOLLIER. Félix Alcan, Paris, 1909. 407 p. (Bibliothèque de Philosophie Contemporaine.)

For the last score of years very much has been written about the will and its re-education, by philosophers, moralists, pedagogues, and psychotherapists, and American pragmatism, which is at bottom a philosophy of will. Indeed, many are coming to believe that truth is what we will. This book is a vigorous reaction against these abuses, which can have nothing but disastrous consequences for morality, pedagogy and therapeutics. To will, it is necessary first of all to know, in order to choose. We must understand the external world and the various physiological and even psychological conditions and consequences. When conditions are not realized, doubt arises because of incertitude and indecision. This is what our age suffers from and really lacks will. At bottom, doubt is an emotive phenomenon due to feeble cerebral resistance. To study it, therefore, now becomes of prime importance for morals and society, to say nothing of pathology and psychology. After some general considerations, the writer takes up in the second chapter the objects and conditions of doubt which, as to the external world, he divides into three parts—that of present, past and future reality; and concerning the ego, he makes three varieties, viz.: as to the object, extent and intent, and time of the doubt. He traces its slow evolution in individual cases and in society, its culmination, decline and disappearance. As to its elements and consequences, he finds them to be affective, sensorial, intellectual, motor; and its causes he traces in sense, in perception, conscience, memory, imagination, association, judgment, feeling, sex, religion, etc. There are certain reactions on the part of doubters that are inherent in the doubt, and dependent upon individual character as well as upon the nature of the doubt itself. Psychæsthenia, obsessions, impulses, pain, are among these. In treating the reactions of the doubter against the doubt, he considers the consequences as intellectually affective and emotive. The organism and nature of doubt he finds in cerebral feebleness and psychæsthenia; and he gives quite a repertory of modes of resistance, beginning with physical exercise, and passing on to psychotherapy and re-education to action.

Zeitschrift für angewandte Psychologie und psychologische Sammelforschung, Band 3, Heft 3 u. 4. Herausgegeben von WILLIAM STERN und OTTO LIPMANN. J. A. Barth, Leipzig, 1909. pp. 163-318.

Among the interesting articles that constitute this number is one contributed by Victor Lowinsky reviewing the *Pedagogical Seminary*, which has always been devoted to education upon a psychological basis and interested in uniting theory and practice. This, the writer says, "has been Stanley Hall's effort, who, with his ethnologically applied bio-genetic law has summoned the whole modern life of culture in all its breadth and depth before the judgment seat of psychology, since although a culture pedagogue *grossten Stiels*, he seeks always to exert an immediately practical influence. He keeps his eye mainly fixed upon the relations of his own country; nevertheless, the scientific issue of his conclusions always has general interest. Of the pathos and ethos of his discussions, this report, of course, can give no intimation. Pedagogical, medical, historical, and psychological points of view are represented." The writer then proceeds to give an epit-

omy of nearly all the leading articles in the *Pedagogical Seminary* during the last two years, including volumes 14 and 15.

Modern Problems in Psychiatry, by ERNESTO LUGARO. Translated by David Orr and R. G. Rows. University Press, Manchester, 1909. 305 p. (Publications of the University of Manchester, Medical Series, No. XII.)

After describing in his general introduction the evolution of psychiatry—its difficulties, the prejudices that hamper it, and breadth of knowledge needful—the author passes first to the psychological problems and stresses the need of distinguishing direct from indirect results of primary disturbance; discusses parallelism, dualism, etc.; is himself a realist; discusses the applications of psychology to the analysis of mental states of the insane and its difficulties. The anatomical problems are then taken up with a good account of Cajal and others. Then follow problems in pathogenesis, etiology and nosology, with a final practical chapter on treatment, asylum problems, relations to crime, etc. The author bases largely on his own experience and has not emerged from the influence of the Kraepelin school.

L'Année Psychologique, publiée par ALFRED BINET avec la collaboration de MM. von Aster, Becher, Benussi, Bergson, Bloch, Borel, etc. Quatorzième Année. Masson et Cie., Paris, 1908. 500 p.

The first article, 94 pages, is by Binet and Simon on the development of attention in children during successive years. Then follows one by Houllevigue on the physical ideas of matter; another by Souriau on sentiment and æsthetics; then follow articles by Borel, on the calculus of probabilities; an inquiry on the history of the methods of teaching philosophy, by Binet; on professional surveillance, by Imbert; on morals and biology, by Rauh; then a criticism of Poincaré by Goblot entitled *Mathematical Demonstration*; another article by Binet and Simon, on language and thought; hygiene and pedagogy, by Chabot; pragmatism, by Cantecor; and Binet on experimental chiromancy. The bibliography is less extensive than usual.

Alte und neue Gehirn-Probleme, von W. W. WENDT. Otto Gmelin, Munich, 1909. 116 p.

The best part of this pamphlet is devoted to the study of brain weights of men and women, ordered according to age, height, and weight. There are also statistics concerning the brains of suicides, those that have met with accidents, and those who have suffered other forms of death. The relation of brain weight to different callings in life and the results of all these tabulations are brought together at the end. The author thinks that the most important result of his statistics is that the brains of lowest weight that have nothing abnormal about them belong usually to day laborers; and here the lightest were 1,120 g., while the lightest academic brain weights were 1,140 g. The author believes that brain weights can be established below which no individual of a certain grade or class ever sinks.

Essai sur la Psychologie de la Main, par N. VASCHIDE. Marcel Rivière, Paris, 1909. 504 p. (Bibliothèque de Philosophie Expérimentale.)

This essay, with thirty-seven full-page plates, is a posthumous work of the brilliant young author who died prematurely two years ago at the age of forty years. It is both comprehensive and unique. Beginning with chiromantic divination, the author proceeds to consider the chiromancy and physiognomy of the hand, from antiquity to the present time; the artistic canons concerning it; he then presents the

history of the hand in art, discusses its anatomy, physiology, psychology, the papillary striations and their value in the identification of criminals, its pathology, writer's cramp, degenerations of the hand, the language of gesture in society, the methods and results of research on the hand, its evolutionary significance, motor images, researches in divination and prevision, and concludes with a special essay on the theory of the possibility of psychic revelations by means of the hand.

Both Sides of the Veil, by ANNE MANNING ROBBINS. Sherman, French & Co., Boston, 1909. 258 p.

This work is introduced by Professor James, and is by a companion of his in psychic research "who from a state of doubt has won through to a faith in human survival in a spiritual order, which continues the visible one. It is a genuine record of a moral and religious experience, profoundly earnest and calculated, I should think, to interest and impress readers who desire to know adequately what deeper significance our life may hold in store." The author evidently lost her creed at Mt Holyoke, soon made the acquaintance of Mrs. Piper, Richard Hodgson, and A. P. Martin; had first failures, then fulfillments. To these the first part is devoted. The second part is entitled Communications from the Other Side of the Veil through Mrs. Piper, with extracts of reports and sittings. The last part is entitled Suggestive Thoughts on the Attainment of Spirituality

A Pluralistic Universe, by WILLIAM JAMES. Longmans, Green & Co., London, 1909. 399 p.

In these carefully studied lectures abounding with all the author's charm of expression, he seeks to "small down the universe" and takes Fechner's notion of a soul of the world as being a deity of sufficient size for humanity to make its deity. To him the whole universe was animated. The earth is in a sense one angel. Plants have souls. His transcendentalism is much above the ordinary pantheism. Consciousness is compounded. We must make a radical breach with intellectualism. The traditional radicalism gives us an essentially static universe. As Bergson, whom this work glorifies, puts it—the function of concepts is practical rather than theoretical. Logic cannot define a universe where change is continuous. Living things are their own others. Empiricism is a better ally of religion than is rationalism. Our beliefs form parts of reality. Indeed the word "rationality" had better be replaced by "intimacy." He wants the basis of discussions for all these questions "broadened and thickened up." He condemns very heartily the Oxford thinkers before whom he lectured for their inane Hegelism, and lashes the German metaphysicians even more severely. In homely terms, the book is a plea that we do not need monism or a unitary view of the world and do not need any theory that has cosmic dimensions; but it is sufficient for us to know the world of which the solar system is the boundary; and all discussions of the absolute and infinite must be abandoned. We have no space to discuss in detail the clever *apperçus* and *bons mots* that sparkle through these pages. It is interesting, however, to note that there could hardly be a more diametrical opposition than between pragmatism, especially the type of it represented by James, and the book of his colleague, Münsterberg, entitled "the Eternal Values," which seems to have been written under the inspiration of the motto: *Delindus est pragmatismus*.

The Meaning of Truth, by WILLIAM JAMES. Longmans, Green & Co., New York, 1909. 298 p.

This volume is an amplification of what the writer calls the pivotal

part of his book on "Pragmatism" and is devoted to the thesis that "the truth is only the expedient in the way of our thinking just as the right is only the expedient in the way of our acting." The present writer believes that most who read this work, unless they are double-dyed speculators as to the abstract question as to what the quiddity of reality actually is, will be interested chiefly in the author's vivacious style which gives a certain sort of interest, even if a meretricious one, to any topic he chooses to consider, and to the fine distinctions he makes between his own views and the various misunderstandings of them that have arisen. The fact that so many intelligent, earnest and respectful, not to say friendly, writers are away off in their interpretation of what James really means and says is itself very significant of the splay-footedness of attempting to treat these serious topics in the off-hand, slap-dash, vivacious way of after-dinner table talk, instead of in the method of severely reasoned, logical thinking that proceeds from point to point, has something that can be called beginning, middle and end, and which is susceptible of proof or disproof, and is met with something more than these tedious, hair-splitting attempts to explain and set others right in a dapper, lively, Hudibrastic way.

Psychology and the Teacher, by HUGO MÜNSTERBERG. D. Appleton & Co., New York, 1909. 340 p.

This is the third book in English of this voluminous author within a few months. In an earlier publication he had declared in a most emphatic way that experimental psychology had nothing in it for teachers. This view he modifies radically here because, as he says, since the first was written new light for education has appeared from the laboratory. But in looking over the thirty-nine chapters, it would be difficult to point out any important theme, the validity of the applications of which was not nearly as well known ten years ago as now. What would have been the harm for this writer frankly to acknowledge a change of view on his own part, which has certainly been both radical and excellent? As for the book itself, it claims, as we understand it, to have nothing whatever new for the psychologist. It is extremely elementary and rehearses what has long been well known. The author also still carefully follows what seems to have long been his policy in refraining from making acknowledgments to other writers, save in a line or two giving a mere list of names. With this point of view it is, for the present writer at least, hard to feel reconciled. The reader should be given a little help on the important topics toward further reading if so disposed. It gives the book an oracular character as though now, for the first time and from this particular author, the truth was vouchsafed to the reader.

Die Funktionen der Nervencentra, von W. v. BECHTEREW. Zweites Heft. Gustav Fischer, Jena, 1909. pp. 695-1336.

This is the second volume of the German translation by Richard Weinberg of the venerable author's probably final revision of his views and account of his own researches. It is devoted to the cerebellum, the mid-brain, and the subcortical ganglia, with 142 cuts.

La Philosophie de S. S. Laurie, par GEORGES REMACLE. M. Weissenbruch and Henri Lamertin, Bruxelles, 1909. 524 p.

All friends of the eminent Scotch educator, born in 1829, will welcome this account of his philosophy. It is divided into two parts: first the theory of knowledge, and second his views of God and man.

Grundriss der Psychologie für Juristen, von OTTO LIPMANN. J. A. Barth, Leipzig, 1908. 80 p.

This book consists of the following lectures: 1. on the essence and method of psychology; 2. the intellectual side of psychic life, including sensations, perceptions, attention, suggestion, association and memory; 3. the side of feeling and will; 4. psychology of *Aussage* as represented by Stern; and 5. the *Tatbestandsdiagnostik* from the standpoint of Freud.

Esthetics, by KATE GORDON. Henry Holt & Co., New York, 1909. 315 p.

This work deals with imagination, feeling, principles of art, rhythm, dancing, music, color, light and form, design, architecture, sculpture, painting, language as an art medium, poetry, drama, prose, and general conceptions of beauty and art. The work certainly does fill a long-felt want, for we know nothing in English that covers this ground.

Elementary Experiments in Psychology, by CARL E. SEASHORE. Henry Holt & Co., New York, 1908. 218 p.

This book makes individual experiments as opposed to class demonstrations practicable, regardless of laboratory facilities or the size of the class. A student is given means and encouragement for pursuing each problem intensively, that he may acquire independence of thought and action, realize the actuality of mental processes, and get here and there a vision of the vastness and orderliness, the practical significance and the charms of the mental life.

The Philosophy of Change, by D. P. RHODES. The Macmillan Co., New York, 1909. 389 p.

In chapter I the author shows the origin of the theory of change in common knowledge. In the succeeding chapters he reviews the principles that underlie human life under the form of an inquiry into the possible destiny of man's various activities, viewed in the light of the past. He next considers the universe of matter and ether, points out the true importance of the problem of reason and will, shows the bearing of this philosophy on the special theory of dissolution, deals with the relation between life and death, discusses the mode of life of any terrestrial race who should hold as rational the view of faith as now attainable. The two last chapters contain random observations upon life as we know it, the uses of rational pessimism, and literary style. It must have given the author great pleasure to solve so neatly so many of the great open questions of the universe.

Wellesley College Studies in Psychology No. 1. A Study in Memorizing Various Materials by the Reconstruction Method, by ELEANOR A. MCC. GAMBLE. The Psychological Publishing Co., Lancaster, Pa., and Baltimore, Md. 210 p. (Psychological Monographs, Vol. X, No. 4, Sept., 1909. Whole No. 43.)

The first chapter states the problems, the second the material (sense, colored paper, nonsense, symbols) with the operations of chance and various limitations. The third gives the method of presenting a series. The fourth deals with the effects of experiments in memorizing upon the serial order of smells and colors. The fifth gives the actual processes involved in memorizing the serial order. The work is carefully, not to say elaborately, done, and is worthy of the author's well-known care and diligence as an investigator.

La Cyclothymie. De la Constitution Cyclothymique et de ses Manifestations, par le DR. PIERRE-KAHN. G. Steinheil, Paris, 1909. 252 p.

This book on intermittent depression and excitation may be said in

some sense to carry on the work of the great Belgian alienist. In the first part he describes the etiology, symptomatology, evolution and diagnostics of excitation. The second part deals with pathogeny and the cyclothymic traits in various mental states and in somatic diseases, especially its relations to gout and diabetes. The subsequent chapters deal with the treatment and with medico-legal considerations and provisions.

Die Weltanschauungen der grossen Philosophen der Neuzeit, von DR. LUDWIG BUSSE. Vierte Auflage herausgegeben von Dr. R. Falckenberg. B. G. Teubner, Berlin, 1909. 156 p. (Aus Natur und Geisteswelt, Sammlung wissenschaftlichgemeinverständlicher Darstellungen. 56. Bändchen.)

The first part deals with the great philosophers before Kant; the second with those since Kant ending with Comte, Mill and Spencer. It is a good little repetitorium for the history of philosophy.

Outlines of Psychiatry, by WILLIAM A. WHITE. Second edition revised and enlarged. The Journal of Nervous and Mental Disease Publishing Co., New York, 1909. 232 p. (Nervous and Mental Disease Monograph Series No. 1.)

This is the second edition of what appears to have been a successful compend dealing with the nature and definition of insanity, classification, cause, treatment, symptoms, examination, paranoia, manic-depressive psychoses, paresis, dementia præcox, involution, melancholia, senile psychoses, infection, exhaustion and toxic psychoses, the psychoses associated with other diseases, borderland and episodic states, with an essay on imbecility.

Consciousness, by HENRY RUTGERS MARSHALL. The Macmillan Co., New York, 1909. 685 p.

This volume compares well with the author's important volume, "Pain, Pleasure and Aesthetics." It is wrought out with great care. The first book deals with consciousness in general—first, that of man; second, that of other than human forms. Book Second is on the general nature of human presentations, with first the general qualities of relation aroused in connection with all presentations; then the general qualities determined by complexity of presentations. Part II is on qualities of relation determined by the correlation of the general qualities studied in Part I. The third book is on the Self.

The Distribution and Functions of Mental Imagery, by GEORGE BETTS. Teachers College, Columbia University, Contributions to Education, No. 26. Teachers College, Columbia University, New York, 1909. 99 p.

The author first experimented with college students and also trained psychologists and then upon spontaneous imagery of various kinds. He has clearly made an important contribution to this now very interesting subject.

Imitation in Monkeys, by M. E. HAGGERTY. Reprinted from the Journal of Comparative Neurology and Psychology, Vol. XIX, No. 4, July, 1909. pp. 337-455.

Popular thought regards imitation as the chief function of monkey life; so that the author's experiments are so important and interesting as to give us much more respect than we had before for the intelligence of monkeys. Those who remember the newspaper notoriety these tests had in New York, where they were made, will be agreeably disappointed to find that they are really scientific.

Logic Inductive and Deductive. An Introduction to Scientific Method.
By ADAM LEROY JONES. Henry Holt & Co., New York, 1909.
304 p.

The contents of this book can be indicated by its chapter heads as follows: first stages of knowledge, classification, use and abuse of words, proposition, induction, verification and deduction; the syllogism—its traditional treatment, abbreviated and complex forms of reasoning, the hypothetical and disjunctive syllogism, proof and disproof. Then follows a special part on supplementary methods, viz.: statistics, averages, probabilities, the construction of systems, hypothesis, typical systems of knowledge.

Problèmes de psychologie affective, Par TH. RIBOT. Paris, F. Alcan, 1910. pp. 172. Price fr. \$2.50.

Professor Ribot has here brought together, with some modification and revision, five of his recent essays on affective problems. The first paper, on affective consciousness (*Rev. philos.*, April, 1909), argues that pleasantness and unpleasantness are but secondary symptoms of the affective life, the true basis of affectivity lying in cœnæsthetic and motor tendencies, and thus in a dimension akin to Wundt's excitement-depression and Royce's restlessness-quietness. The second, on affective memory (*ibid.*, Dec., 1907; *cf. Journ. de psych.*, July-August, 1909), brings additional evidence for the writer's theory of a purely affective memory,—evidence which will be accepted or rejected according to the reader's definition of affective process. The third, a study of antipathy (*Rev. philos.*, Nov., 1908), is especially valuable, as breaking what is practically virgin soil in psychology. The fourth, on the nature of pleasure (*ibid.*, August, 1909), is rather critical than constructive, although the author gives fresh formulation to his well-known doctrine that pleasure is the conscious translation of the fact of successful functioning on the part of the physical or mental constitution. The concluding essay, on a form of affective illusion (*ibid.*, May, 1907), discusses certain cases of *misfelt* (if that term may be coined) and misinterpreted feeling, as the euphoria of the seriously ill, or our belief that we desire a visit the postponement of which reveals that we had been looking forward to it with aversion.

Professor Ribot gives a half-promise, in his preface, of a future work on affective theory, to be entitled *La vie affective et les mouvements*. Such a book, written with critical regard to the definitions and usages of other contemporary psychologists, would be of extreme interest. As it is, one is often puzzled to say whether one agrees or disagrees with the writer, since he nowhere takes up a definite position as to the dividing line between affective and sensational experience. Thus, though he rules out kinæsthesia, warmth and cold (with their derivatives) from any share in the strictly affective life, he nevertheless regards cœnæsthesia as purely affective in character, and makes affection essentially motor in derivation. Even a negative demarcation, the statement that such and such organic experiences are sensory and consequently non-affective, if made with sufficient definiteness and rigidly adhered to, would be of great assistance in clearing up the points upon which Professor Ribot agrees with and differs from other students of the subject. It is a further source of confusion that the essays slip back and forth, without warning, between the biological and the psychological standpoints.

FRANCIS JONES.

Genetic Psychology: an Introduction to an Objective and Genetic View of Intelligence. By E. A. KIRKPATRICK. New York, The Macmillan Co., 1909. pp. xv., 373.

"In working out the general principles of mental genesis it was found that the more specific problem of mental phenomena as organized in individual minds could not be satisfactorily treated till the general truths of organic activity and of mental genesis had been formulated, and that space would not admit of the treatment of both in a single volume. The author hopes to treat of mental development in individuals and some of the pedagogical implications in a subsequent volume." The present work, which may thus be considered as the first volume of a complete Genetic Psychology, contains eleven chapters. Chapter i., Introduction, discusses the nature and scope of genetic psychology, the general characteristics of organisms, evolution and the genesis of behavior and of mind, the psychological factor in behavior. Chapter ii. treats of the structural basis of behavior, and Chapter iii. of types of animal behavior. Chapters iv. and v. consider instinct, that is, complex behavior characteristic of species, and the acquisition of habits and ideas, that is, the behavior of individuals; and Chapter vi. sets forth the structures concerned in complex behavior and in ideation. Chapter vii., Consciousness, discusses the objective and subjective criteria of consciousness, and the probable general characteristics of the consciousness of animals; Chapter viii. characterizes some specific conscious states (pain, intellectual states, volitional activity). Chapters ix. and x. deal with types of adaptive activity or intelligence, and types of learning activity; and the concluding Chapter xi. outlines the general theory and principles of racial and individual development.

The work originated in lecture-courses given to summer students at Columbia and Chicago universities, and parts of it have been read as lectures in a normal school. In origin and plan of treatment, the author says, pedagogical interest has played a large part. "It is probable, also, that popular interest is great enough to make the book acceptable to the more serious of those interested in animal behavior." On the scientific side, "it is hoped that this tentative formulation of the truths supplied by various sciences may help to promote more harmony of effort on the part of the workers in the several related fields."

However, as a scientific work, the *Genetic Psychology* cannot compare with Washburn's *Animal Mind*. Probably, indeed, the author would resent the comparison. It seems fairer to consider the book as a frankly popular work, intended to catch the interest of the beginning student and of the general educated public, and to lead on beyond itself to a strictly scientific interest in the problems of mental development. If we regard it in this way, we have a readable work, logically planned, and kept almost always at the level of common-sense intelligence, so that it may be understood without effort. Interest is maintained by illustrations, anecdotes and examples; and that the interest may at once find sustenance, a selected bibliography of easily accessible sources is appended to every chapter.

If we apply scientific standards, on the other hand, the author must be convicted of looseness of argument and over-hasty generalization. Take, for instance, the concrete case by which he seeks to demonstrate the mode of operation of natural selection (p. 9). "Why are not sparrows as large as geese or as small as flies instead of being just about the size they are? After a severe storm a large number of sparrows were picked up in a helpless condition and as many as possible resuscitated. All were then weighed and measured to determine

the size and proportion of parts. It was found that nearly all that varied to any considerable degree from the average were dead, while most of those near the average in all respects were alive. This of course means that a certain size and proportion of parts are most favorable for a sparrow living in this climate, and that those birds that vary the most from the normal are likely to be destroyed and produce few or no descendants. In a different environment or living a different life, the size and proportion of parts of the sparrow might be different, but in any case environment determines through natural selection the size of sparrows living in a certain way in a certain place." We notice, first of all, that no definition is given of a 'part', and that the 'average' used is not specified; however, it is probable that external measurements only were made, and that the average is the arithmetical mean. We notice, secondly, the implication that sparrow size and sparrow proportion of parts may differ in different environments and with different modes of life; the implication is left vague, without reference to the facts of variation; so that the thoughtless reader might actually conceive of sparrows, in some outlandish quarter of the world, as large as geese or as small as flies. These are minor points of criticism. It is more important to note that the argument as a whole is unsound. "Once sparrow-size has been fixed (whether by the direct action of environment, by the operation of natural selection, by intrinsic laws of growth, or by any other causal agency or combination of causal agencies), then deviation from the normal will probably bring with it a constitutional weakness; over-large sparrows, *e.g.*, may have too small hearts, over-small sparrows may have an inadequate musculature, and so forth. The experiment brings no evidence whatsoever that "a certain size and proportion of parts are most favorable for a sparrow living in this climate"; it brings no evidence, that is, that a sparrow as large as a robin (if such a bird existed) could not propagate its kind, in the climate referred to, as well as or even better than the existing species; it shows only that, when once size has been fixed, considerable departure from that size, on the part of the individual, means unbalance of the vital functions. The question "why sparrows are not as large as geese or as small as flies" is not touched; the illustration proves, at most, that a size once fixed is maintained.

The passage quoted continues as follows: "What is true of the size of the sparrow is true of all his other characteristics, including his behavior; for if he attempted to do what his structure does not fit him to do, he would be at a disadvantage and would be eliminated by natural selection." Yet on p. 11 we read of "changes in the mode of behavior of different groups of the common species, which *necessitated corresponding changes in structure*" (italics not in original). Slips of this sort are not uncommon.

Consider again the following passages. "In studying the structure and behavior of various types of organisms we must interpret them as means of survival for the individual or for the species" (p. 10); "it is reasonable to suppose that this [consciousness] *like all other characteristics of organisms*, is the product of natural selection, and that it has been preserved in certain kinds of behavior of certain organisms because it has proved useful" (pp. 13 f.; italics not in original). Yet the biologists themselves are tending more and more to emphasize the occurrence of non-adaptive variations. And apart from the question of the all-sufficiency of natural selection, the author forgets that utility is not a scientific concept. The natural world is a world of causation; and when we have explained in terms of cause and effect, our explanatory task is done. Huxley says somewhere that

the *Origin of Species* appeared to him, as he read it, to give the death blow to teleology in the domain of life-phenomena. Teleology has, unfortunately, come to life again, and flourishes all too abundantly in the pages of this book.

The treatment of consciousness is vacillating (13 f., 190 f., 257 ff., etc.). There is ground for making consciousness a form of energy, and using it as such in causal explanation. It is legitimate also to adopt the working principle of parallelism, and to leave consciousness out of account in dealing with the material world of physics and physiology. Both positions are defended by competent psychologists, and both have weighty arguments in their favor. The author takes a sort of middle course between the two views. Consciousness is the product of natural selection, and has been preserved because it is useful; yet "it is safer not to *assume* conscious states until all the simpler and more demonstrable factors in behavior have been given due weight" (p. 13). Why 'simpler'? Why should consciousness be a complex factor in behavior? "In general, the function of consciousness is not to actually do things but to adjust apparatus for doing them, note the results, and readjust as needed" (p. 205). The function would, then, be simple or complex according as the apparatus are crude or delicate. Cf. p. 178: "in racial development consciousness in the form of rudimentary feeling indicating the necessity or non-necessity of movement may be present momentarily even in the lowest organisms and help to produce more prompt and effective reactions." Here the function of consciousness would be extremely simple; nothing more, in fact, than the saying of Yes or No to a motor impulse.

A final word may be said with regard to the writer's four levels of adaptive activity or 'intelligence'. First in order comes physiological intelligence, shown especially in nutritive and growth processes, and concerned with the direction of activities taking place within the body in such a way as to preserve life. Next follows sensory-motor intelligence, with or without consciousness, directing movements of a part or of the whole body in response to external stimulation in such a way that favorable results may be secured. In the third place stands representative intelligence, of which imagination and memory are characteristic manifestations, making possible economy of movement, ministering to psychical needs, and teaching by way of imitation. Last of all comes conceptual intelligence, which not only makes it possible to accomplish certain purposes more quickly and effectively than by sensory motor or representative intelligence, and to meet new situations which could not be reacted to effectively by any other form of intellectual process, but also enables its possessor to go beyond what can be experienced or even represented. It seems clear that we have, in this hierarchy of faculties, a logical construction of the kind dear to Romanes and his contemporaries, rather than an actual picture of the course of mental evolution.

It is natural for a critic, writing in a technical journal, to estimate the worth of a book from the purely scientific point of view. The present writer can, however, conceive (as he indicated above) that the *Genetic Psychology*, despite all its sins of scientific omission and commission, may serve a very useful purpose as a popular introduction to its subject. Many a student has been attracted to a science by some general treatise, to which he will acknowledge a real debt of gratitude on that account, even though in later years he reject or essentially modify its teaching.

OTTO PERLER.

Die Kitzel-und Juckempfindungen. Von S. ALRUTZ. Separat-Abdruck aus dem Skandinavischen Archiv für Physiologie, xx, 1908, 371-410.

The recent work of Török (*Zeits. f. Psych.*, xlv) made it appear fairly certain that superficial tickle is to be referred to the pressure organs, and itch to the organs of cutaneous pain. The probability of this ascription was strengthened by Murray's failure (this *Journal*, xix) to confirm Alrutz' observation, of which more was made by Thunberg than by Alrutz himself, that sensations of tickle may be aroused in the regions of the skin lying between the pressure-spots and consequently insensitive to pressure. In his present paper, however, Alrutz continues to identify tickle and itch. It is impossible, he says, to draw a sharp introspective distinction between the two; there are times and places when you cannot decide which you are experiencing; there are, that is, transitions between the extreme forms. There is no introspective resemblance between either tickle or itch and light pressure (contact). Nor is there any observable likeness between tickle and prick. Itch, on the other hand, is like prick and may pass over into prick by intermediate stages.

After a review of previous work, normal, pathological and clinical, and after reporting further experiments of his own, Alrutz decides positively that itch has nothing to do with pressure. He decides, also, that it is as good as proved that tickle is altogether independent of the pressure sense. Tickle and itch thus appear as one and the same quality, differently named according to the mode of stimulation (external or internal) of the organ, of a distinct sense. The organ itself is probably a less differentiated form of the pain organ. He adds that punctiform pain or prick, and diffuse cutaneous pain or smart, come from the same terminal organ, namely the cutaneous organ of pain.

It is difficult to see how these conclusions are to be reconciled with the results of Murray's work upon tickle. In the reviewer's opinion, Alrutz is not justified in identifying the qualities of tickle and of itch.

P. E. WINTER.

Those Nerves, by GEORGE LINCOLN WALTON. J. B. Lippincott Co., Philadelphia, 1909. 203 p.

This is a very chatty series of very informal talks, especially on various obsessions and physical habits and their control. The object of the book is to promote such habits of mind as may make for health as well as happiness. It is preventive.

NOTE

The life and work of Liégeois, professor of law, for many years in the University of Nancy and one of the founders, with Liébault and Bernheim, of the "Nancy School" of hypnotism, has been fittingly commemorated by the erection of a monument by international subscription, in his native city of Damvillers. The monument takes the form of a bust upon a pedestal of granite and stands in the square before the city hall. The formal exercises of commemoration took place on Oct. 24, 1909, in the presence of official delegates from the University of Nancy, the Institute of France, the Society of Hypnotology, and men of science of France and other countries. Addresses were delivered by MM. Charles Humbert, Van Reuterghem Bérillon, Bonjeau, Lyon, Caen the mayor of Damvillers and others. Liégeois besides doing successful work in economics, has put both psychologists and jurists in his debt by his pioneer studies of hypnotism and suggestion in their medico-legal aspects.

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THE ORIGIN AND DEVELOPMENT OF PSYCHOANALYSIS¹

By PROF. SIGMUND FREUD (Vienna)

FIRST LECTURE

Ladies and Gentlemen: It is a new and somewhat embarrassing experience for me to appear as lecturer before students of the New World. I assume that I owe this honor to the association of my name with the theme of psychoanalysis, and consequently it is of psychoanalysis that I shall aim to speak. I shall attempt to give you in very brief form an historical survey of the origin and further development of this new method of research and cure.

Granted that it is a merit to have created psychoanalysis, it is not my merit. I was a student, busy with the passing of my last examinations, when another physician of Vienna, Dr. Joseph Breuer,² made the first application of this method to the case of an hysterical girl (1880-82). We must now examine the history of this case and its treatment, which can be found in detail in "Studien über Hysterie," later published by Dr. Breuer and myself.³

But first one word. I have noticed, with considerable satis-

¹ Lectures delivered at the Celebration of the Twentieth Anniversary of the opening of Clark University, Sept., 1909; translated from the German by Harry W. Chase, Fellow in Psychology, Clark University, and revised by Prof. Freud.

² Dr. Joseph Breuer, born 1842, corresponding member of the "Kaiserliche Akademie der Wissenschaften," is known by works on respiration and the physiology of the sense of equilibrium.

³ "Studien über Hysterie," 1895, Deuticke, Vienna. Second edition, 1909. Parts of my contributions to this book have been translated into English by Dr. A. A. Brill, of New York. ("Selected Papers on Hysteria and other Psychoneuroses, by S. Freud.")

faction, that the majority of my hearers do not belong to the medical profession. Now do not fear that a medical education is necessary to follow what I shall have to say. We shall now accompany the doctors a little way, but soon we shall take leave of them and follow Dr. Breuer on a way which is quite his own.

Dr. Breuer's patient was a girl of twenty-one, of a high degree of intelligence. She had developed in the course of her two years' illness a series of physical and mental disturbances which well deserved to be taken seriously. She had a severe paralysis of both right extremities, with anasthesia, and at times the same affection of the members of the left side of the body; disturbance of eye-movements, and much impairment of vision; difficulty in maintaining the position of the head, an intense *Tussis nervosa*, nausea when she attempted to take nourishment, and at one time for several weeks a loss of the power to drink, in spite of tormenting thirst. Her power of speech was also diminished, and this progressed so far that she could neither speak nor understand her mother tongue; and, finally, she was subject to states of "absence," of confusion, delirium, alteration of her whole personality. These states will later claim our attention.

When one hears of such a case, one does not need to be a physician to incline to the opinion that we are concerned here with a serious injury, probably of the brain, for which there is little hope of cure and which will probably lead to the early death of the patient. The doctors will tell us, however, that in one type of cases with just as unfavorable symptoms, another, far more favorable, opinion is justified. When one finds such a series of symptoms in the case of a young girl, whose vital organs (heart, kidneys), are shown by objective tests to be normal, but who has suffered from strong emotional disturbances, and when the symptoms differ in certain finer characteristics from what one might logically expect, in a case like this the doctors are not too much disturbed. They consider that there is present no organic lesion of the brain, but that enigmatical state, known since the time of the Greek physicians as hysteria, which can simulate a whole series of symptoms of various diseases. They consider in such a case that the life of the patient is not in danger and that a restoration to health will probably come about of itself. The differentiation of such an hysteria from a severe organic lesion is not always very easy. But we do not need to know how a differential diagnosis of this kind is made; you may be sure that the case of Breuer's patient was such that no skillful physician could fail to diagnose an hysteria. We may also add a word here from the history of the case. The illness first appeared while the

patient was caring for her father, whom she tenderly loved, during the severe illness which led to his death, a task which she was compelled to abandon because she herself fell ill.

So far it has seemed best to go with the doctors, but we shall soon part company with them. You must not think that the outlook of a patient with regard to medical aid is essentially bettered when the diagnosis points to hysteria rather than to organic disease of the brain. Against the serious brain diseases medical skill is in most cases powerless, but also in the case of hysterical affections the doctor can do nothing. He must leave it to benign nature, when and how his hopeful prognosis will be realized.¹ Accordingly, with the recognition of the disease as hysteria, little is changed in the situation of the patient, but there is a great change in the attitude of the doctor. We can observe that he acts quite differently toward hystericals than toward patients suffering from organic diseases. He will not bring the same interest to the former as to the latter, since their suffering is much less serious and yet seems to set up the claim to be valued just as seriously.

But there is another motive in this action. The physician, who through his studies has learned so much that is hidden from the laity, can realize in his thought the causes and alterations of the brain disorders in patients suffering from apoplexy or dementia, a representation which must be right up to a certain point, for by it he is enabled to understand the nature of each symptom. But before the details of hysterical symptoms, all his knowledge, his anatomical-physiological and pathological education, desert him. He cannot understand hysteria. He is in the same position before it as the layman. And that is not agreeable to any one, who is in the habit of setting such a high valuation upon his knowledge. Hystericals, accordingly, tend to lose his sympathy; he considers them persons who overstep the laws of his science, as the orthodox regard heretics; he ascribes to them all possible evils, blames them for exaggeration and intentional deceit, "simulation," and he punishes them by withdrawing his interest.

Now Dr. Breuer did not deserve this reproach in this case; he gave his patient sympathy and interest, although at first he did not understand how to help her. Probably this was easier for him on account of those superior qualities of the patient's mind and character, to which he bears witness in his account of the case.

His sympathetic observation soon found the means which

¹I know that this view no longer holds to-day, but in the lecture I take myself and my hearers back to the time before 1880. If things have become different since that time it has been largely due to the work the history of which I am sketching.

made the first help possible. It had been noticed that the patient, in her states of "absence," of psychic alteration, usually mumbled over several words to herself. These seemed to spring from associations with which her thoughts were busy. The doctor, who was able to get these words, put her in a sort of hypnosis and repeated them to her over and over, in order to bring up any associations that they might have. The patient yielded to his suggestion and reproduced for him those psychic creations which controlled her thoughts during her "absences," and which betrayed themselves in these single spoken words. These were fancies, deeply sad, often poetically beautiful, day dreams, we might call them, which commonly took as their starting point the situation of a girl beside the sick-bed of her father. Whenever she had related a number of such fancies, she was, as it were, freed and restored to her normal mental life. This state of health would last for several hours, and then give place on the next day to a new "absence," which was removed in the same way by relating the newly-created fancies. It was impossible not to get the impression that the psychic alteration which was expressed in the "absence" was a consequence of the excitations originating from these intensely emotional fancy-images. The patient herself, who at this time of her illness strangely enough understood and spoke only English, gave this new kind of treatment the name "talking cure," or jokingly designated it as "chimney sweeping."

The doctor soon hit upon the fact that through such cleansing of the soul more could be accomplished than a temporary removal of the constantly recurring mental "clouds." Symptoms of the disease would disappear when in hypnosis the patient could be made to remember the situation and the associative connections under which they first appeared, provided free vent was given to the emotions which they aroused. "There was in the summer a time of intense heat, and the patient had suffered very much from thirst; for, without any apparent reason, she had suddenly become unable to drink. She would take a glass of water in her hand, but as soon as it touched her lips she would push it away as though suffering from hydrophobia. Obviously for these few seconds she was in her absent state. She ate only fruit, melons and the like, in order to relieve this tormenting thirst. When this had been going on about six weeks, she was talking one day in hypnosis about her English governess, whom she disliked, and finally told, with every sign of disgust, how she had come into the room of the governess, and how that lady's little dog, that she abhorred, had drunk out of a glass. Out of respect for the conventions the patient had remained silent. Now,

after she had given energetic expression to her restrained anger, she asked for a drink, drank a large quantity of water without trouble, and woke from hypnosis with the glass at her lips. The symptom thereupon vanished permanently."¹

Permit me to dwell for a moment on this experience. No one had ever cured an hysterical symptom by such means before, or had come so near understanding its cause. This would be a pregnant discovery if the expectation could be confirmed that still other, perhaps the majority of symptoms, originated in this way and could be removed by the same method. Breuer spared no pains to convince himself of this and investigated the pathogenesis of the other more serious symptoms in a more orderly way. Such was indeed the case; almost all the symptoms originated in exactly this way, as remnants, as precipitates, if you like, of affectively-toned experiences, which for that reason we later called "psychic traumata." The nature of the symptoms became clear through their relation to the scene which caused them. They were, to use the technical term, "determined" (*determiniert*) by the scene whose memory traces they embodied, and so could no longer be described as arbitrary or enigmatical functions of the neurosis.

Only one variation from what might be expected must be mentioned. It was not always a single experience which occasioned the symptom, but usually several, perhaps many similar, repeated traumata co-operated in this effect. It was necessary to repeat the whole series of pathogenic memories in chronological sequence, and of course in reverse order, the last first and the first last. It was quite impossible to reach the first and often most essential trauma directly, without first clearing away those coming later.

You will of course want to hear me speak of other examples of the causation of hysterical symptoms beside this of inability to drink on account of the disgust caused by the dog drinking from the glass. I must, however, if I hold to my programme, limit myself to very few examples. Breuer relates, for instance, that his patient's visual disturbances could be traced back to external causes, in the following way. "The patient, with tears in her eyes, was sitting by the sick-bed when her father suddenly asked her what time it was. She could not see distinctly, strained her eyes to see, brought the watch near her eyes so that the dial seemed very large (*macropia* and *strabismus conv.*), or else she tried hard to suppress her tears, so that the sick man might not see them."²

All the pathogenic impressions sprang from the time when

¹ "Studien über Hysterie," 2d edition, p. 26.

² "Studien über Hysterie," 2d edition, p. 31.

she shared in the care of her sick father. "Once she was watching at night in the greatest anxiety for the patient, who was in a high fever, and in suspense, for a surgeon was expected from Vienna, to operate on the patient. Her mother had gone out for a little while, and Anna sat by the sick-bed, her right arm hanging over the back of her chair. She fell into a reverie and saw a black snake emerge, as it were, from the wall and approach the sick man as though to bite him. (It is very probable that several snakes had actually been seen in the meadow behind the house, that she had already been frightened by them, and that these former experiences furnished the material for the hallucination.) She tried to drive off the creature, but was as though paralyzed. Her right arm, which was hanging over the back of the chair, had "gone to sleep," become anesthetic and paretic, and as she was looking at it, the fingers changed into little snakes with deaths-heads. (The nails.) Probably she attempted to drive away the snake with her paralyzed right hand, and so the anesthesia and paralysis of this member formed associations with the snake hallucination. When this had vanished, she tried in her anguish to speak, but could not. She could not express herself in any language, until finally she thought of the words of an English nursery song, and thereafter she could think and speak only in this language."¹ When the memory of this scene was revived in hypnosis the paralysis of the right arm, which had existed since the beginning of the illness, was cured and the treatment ended.

When, a number of years later, I began to use Breuer's researches and treatment on my own patients, my experiences completely coincided with his. In the case of a woman of about forty, there was a tic, a peculiar smacking noise which manifested itself whenever she was laboring under any excitement, without any obvious cause. It had its origin in two experiences which had this common element, that she attempted to make no noise, but that by a sort of counter-will this noise broke the stillness. On the first occasion, she had finally after much trouble put her sick child to sleep, and she tried to be very quiet so as not to awaken it. On the second occasion, during a ride with both her children in a thunderstorm the horses took fright, and she carefully avoided any noise for fear of frightening them still more.² I give this ex-

¹ "Studien über Hysterie," 2d edition, p. 30.

² *Loc cit.*, 2d ed. pp. 43-46. A selection from this book, augmented by several later treatises on hysteria, lies before me, in an English translation by Dr. A. A. Brill, of New York. It bears the title "Selected Papers on Hysteria and other Psychoneuroses," 1909. [No. 4 of the Nervous and Mental Disease Monograph Series, New York.]

ample instead of many others which are cited in the "Studien über Hysterie."

Ladies and gentlemen, if you will permit me to generalize, as is indispensable in so brief a presentation, we may express our results up to this point in the formula: *Our hysterical patients suffer from reminiscences.* Their symptoms are the remnants and the memory symbols of certain (traumatic) experiences.

A comparison with other memory symbols from other sources will perhaps enable us better to understand this symbolism. The memorials and monuments with which we adorn our great cities, are also such memory symbols. If you walk through London you will find before one of the greatest railway stations of the city a richly decorated Gothic pillar—"Charing Cross." One of the old Plantagenet kings, in the thirteenth century, caused the body of his beloved queen Eleanor to be borne to Westminster, and had Gothic crosses erected at each of the stations where the coffin was set down. Charing Cross is the last of these monuments, which preserve the memory of this sad journey.¹ In another part of the city, you will see a high pillar of more modern construction, which is merely called "the monument." This is in memory of the great fire which broke out in the neighborhood in the year 1666, and destroyed a great part of the city. These monuments are memory symbols like the hysterical symptoms; so far the comparison seems justified. But what would you say to a Londoner who to-day stood sadly before the monument to the funeral of Queen Eleanor, instead of going about his business with the haste engendered by modern industrial conditions, or rejoicing with the young queen of his own heart? Or to another, who before the "Monument" bemoaned the burning of his loved native city, which long since has arisen again so much more splendid than before?

Now hystericals and all neurotics behave like these two unpractical Londoners, not only in that they remember the painful experiences of the distant past, but because they are still strongly affected by them. They cannot escape from the past and neglect present reality in its favor. This fixation of the mental life on the pathogenic traumata is an essential, and practically a most significant characteristic of the neurosis. I will willingly concede the objection which you are probably formulating, as you think over the history of Breuer's patient. All her traumata originated at the time when she was caring for her sick father, and her symptoms could only be regarded as memory symbols of his sickness and death. They corre-

¹ Or rather the later copy of such a monument. The name "Charing" is itself, as Dr. E. Jones tells me, derived from the words "*chère reine.*"

sponded to mourning, and a fixation on thoughts of the dead so short a time after death is certainly not pathological, but rather corresponds to normal emotional behavior. I concede this: there is nothing abnormal in the fixation of feeling on the trauma shown by Breuer's patient. But in other cases, like that of the tic that I have mentioned, the occasions for which lay ten and fifteen years back, the characteristic of this abnormal clinging to the past is very clear, and Breuer's patient would probably have developed it, if she had not come under the "cathartic treatment" such a short time after the traumatic experiences and the beginning of the disease.

We have so far only explained the relation of the hysterical symptoms to the life history of the patient; now by considering two further moments which Breuer observed, we may get a hint as to the processes of the beginning of the illness and those of the cure. With regard to the first, it is especially to be noted that Breuer's patient in almost all pathogenic situations had to suppress a strong excitement, instead of giving vent to it by appropriate words and deeds. In the little experience with her governess' dog, she suppressed, through regard for the conventions, all manifestations of her very intense disgust. While she was seated by her father's sick bed, she was careful to betray nothing of her anxiety and her painful depression to the patient. When, later, she reproduced the same scene before the physician, the emotion which she had suppressed on the occurrence of the scene burst out with especial strength, as though it had been pent up all along. The symptom which had been caused by that scene reached its greatest intensity while the doctor was striving to revive the memory of the scene, and vanished after it had been fully laid bare. On the other hand, experience shows that if the patient is reproducing the traumatic scene to the physician, the process has no curative effect if, by some peculiar chance, there is no development of emotion. It is apparently these emotional processes upon which the illness of the patient and the restoration to health are dependent. We feel justified in regarding "emotion" as a quantity which may become increased, derived and displaced. So we are forced to the conclusion that the patient fell ill because the emotion developed in the pathogenic situation was prevented from escaping normally, and that the essence of the sickness lies in the fact that these "imprisoned" (*dingeklemmt*) emotions undergo a series of abnormal changes. In part they are preserved as a lasting charge and as a source of constant disturbance in psychical life; in part they undergo a change into unusual bodily innervations and inhibitions, which present themselves as the physical symptoms of the case. We have coined the name "hysterical

conversion" for the latter process. Part of our mental energy is, under normal conditions, conducted off by way of physical innervation and gives what we call "the expression of emotions." Hysterical conversion exaggerates this part of the course of a mental process which is emotionally colored; it corresponds to a far more intense emotional expression, which finds outlet by new paths. If a stream flows in two channels, an overflow of one will take place as soon as the current in the other meets with an obstacle.

You see that we are in a fair way to arrive at a purely psychological theory of hysteria, in which we assign the first rank to the affective processes. A second observation of Breuer compels us to ascribe to the altered condition of consciousness a great part in determining the characteristics of the disease. His patient showed many sorts of mental states, conditions of "absence," confusion and alteration of character, besides her normal state. In her normal state she was entirely ignorant of the pathogenic scenes and of their connection with her symptoms. She had forgotten those scenes, or at any rate had dissociated them from their pathogenic connection. When the patient was hypnotized, it was possible, after considerable difficulty, to recall those scenes to her memory, and by this means of recall the symptoms were removed. It would have been extremely perplexing to know how to interpret this fact, if hypnotic practice and experiments had not pointed out the way. Through the study of hypnotic phenomena, the conception, strange though it was at first, has become familiar, that in one and the same individual several mental groupings are possible, which may remain relatively independent of each other, "know nothing" of each other, and which may cause a splitting of consciousness along lines which they lay down. Cases of such a sort, known as "double personality" ("*double conscience*"), occasionally appear spontaneously. If in such a division of personality consciousness remains constantly bound up with one of the two states, this is called the *conscious* mental state, and the other the *unconscious*. In the well-known phenomena of so-called post hypnotic suggestion, in which a command given in hypnosis is later executed in the normal state as though by an imperative suggestion, we have an excellent basis for understanding how the unconscious state can influence the conscious, although the latter is ignorant of the existence of the former. In the same way it is quite possible to explain the facts in hysterical cases. Breuer came to the conclusion that the hysterical symptoms originated in such peculiar mental states, which he called "hypnoidal states." (*hypnoide Zustände*.) Experiences of an emotional nature, which occur dur-

ing such hypnoidal states easily become pathogenic, since such states do not present the conditions for a normal draining off of the emotion of the exciting processes. And as a result there arises a peculiar product of this exciting process, that is, the symptom, and this is projected like a foreign body into the normal state. The latter has, then, no conception of the significance of the hypnoidal pathogenic situation. Where a symptom arises, we also find an amnesia, a memory gap, and the filling of this gap includes the removal of the conditions under which the symptom originated.

I am afraid that this portion of my treatment will not seem very clear, but you must remember that we are dealing here with new and difficult views, which perhaps could not be made much clearer. This all goes to show that our knowledge in this field is not yet very far advanced. Breuer's idea of the hypnoidal states has, moreover, been shown to be superfluous and a hindrance to further investigation, and has been dropped from present conceptions of psychoanalysis. Later I shall at least suggest what other influences and processes have been disclosed besides that of the hypnoidal states, to which Breuer limited the causal moment.

You have probably also felt, and rightly, that Breuer's investigations gave you only a very incomplete theory and insufficient explanation of the phenomena which we have observed. But complete theories do not fall from Heaven, and you would have had still greater reason to be distrustful, had any one offered you at the beginning of his observations a well-rounded theory, without any gaps; such a theory could only be the child of his speculations and not the fruit of an unprejudiced investigation of the facts.

SECOND LECTURE

Ladies and Gentlemen: At about the same time that Breuer was using the "talking-cure" with his patient, M. Charcot began in Paris, with the hystericals of the Salpêtrière, those researches which were to lead to a new understanding of the disease. These results were, however, not yet known in Vienna. But when about ten years later Breuer and I published our preliminary communication on the psychic mechanism of hysterical phenomena, which grew out of the cathartic treatment of Breuer's first patient, we were both of us under the spell of Charcot's investigations. We made the pathogenic experiences of our patients, which acted as psychic traumata, equivalent to those physical traumata whose influence on hysterical paralyses Charcot had determined; and Breuer's hypothesis of hypnoidal states is itself only an echo of the fact

that Charcot had artificially reproduced those traumatic paralysees in hypnosis.

The great French observer, whose student I was during the years 1885-86, had no natural bent for creating psychological theories. His student, P. Janet, was the first to attempt to penetrate more deeply into the psychic processes of hysteria, and we followed his example, when we made the mental splitting and the dissociation of personality the central points of our theory. Janet propounds a theory of hysteria which draws upon the principal theories of heredity and degeneration which are current in France. According to his view hysteria is a form of degenerative alteration of the nervous system, manifesting itself in a congenital "weakness" of the function of psychic synthesis. The hysterical patient is from the start incapable of correlating and unifying the manifold of his mental processes, and so there arises the tendency to mental dissociation. If you will permit me to use a banal but clear illustration, Janet's hysterical reminds one of a weak woman who has been shopping, and is now on her way home, laden with packages and bundles of every description. She cannot manage the whole lot with her two arms and her ten fingers, and soon she drops one. When she stoops to pick this up, another breaks loose, and so it goes on.

Now it does not agree very well with this assumed mental weakness of hystericals, that there can be observed in hysterical cases, besides the phenomena of lessened functioning, examples of a partial increase of functional capacity, as a sort of compensation. At the time when Breuer's patient had forgotten her mother-tongue and all other languages save English, her control of English attained such a level that if a German book was put before her she could give a fluent, perfect translation of its contents at sight. When later I undertook to continue on my own account the investigations begun by Breuer, I soon came to another view of the origin of hysterical dissociation (or splitting of consciousness). It was inevitable that my views should diverge widely and radically, for my point of departure was not, like that of Janet, laboratory researches, but attempts at therapy. Above everything else, it was practical needs that urged me on. The cathartic treatment, as Breuer had made use of it, presupposed that the patient should be put in deep hypnosis, for only in hypnosis was available the knowledge of his pathogenic associations, which were unknown to him in his normal state. Now hypnosis, as a fanciful, and so to speak, mystical, aid, I soon came to dislike; and when I discovered that, in spite of all my efforts, I could not hypnotize by any means all of my patients, I resolved to give up hypnotism and to make the cathartic method independent of it.

Since I could not alter the psychic state of most of my patients at my wish, I directed my efforts to working with them in their normal state. This seems at first sight to be a particularly senseless and aimless undertaking. The problem was this: to find out something from the patient that the doctor did not know and the patient himself did not know. How could one hope to make such a method succeed? The memory of a very noteworthy and instructive proceeding came to my aid, which I had seen in Bernheim's clinic at Nancy. Bernheim showed us that persons put in a condition of hypnotic somnambulism, and subjected to all sorts of experiences, had only apparently lost the memory of those somnambulatory experiences, and that their memory of them could be awakened even in the normal state. If he asked them about their experiences during somnambulism, they said at first that they did not remember, but if he persisted, urged, assured them that they did know, then every time the forgotten memory came back.

Accordingly I did this with my patients. When I had reached in my procedure with them a point at which they declared that they knew nothing more, I would assure them that they did know, that they must just tell it out, and I would venture the assertion that the memory which would emerge at the moment that I laid my hand on the patient's forehead would be the right one. In this way I succeeded, without hypnosis, in learning from the patient all that was necessary for a construction of the connection between the forgotten pathogenic scenes and the symptoms which they had left behind. This was a troublesome and in its length an exhausting proceeding, and did not lend itself to a finished technique. But I did not give it up without drawing definite conclusions from the data which I had gained. I had substantiated the fact that the forgotten memories were not lost. They were in the possession of the patient, ready to emerge and form associations with his other mental content, but hindered from becoming conscious, and forced to remain in the unconscious by some sort of a force. The existence of this force could be assumed with certainty, for in attempting to drag up the unconscious memories into the consciousness of the patient, in opposition to this force, one got the sensation of his own personal effort striving to overcome it. One could get an idea of this force, which maintained the pathological situation, from the resistance of the patient.

It is on this idea of *resistance* that I based my theory of the psychic processes of hystericals. It had been found that in order to cure the patient it was necessary that this force should be overcome. Now with the mechanism of the cure as

a starting point, quite a definite theory could be constructed. These same forces, which in the present situation as resistances opposed the emergence of the forgotten ideas into consciousness, must themselves have caused the forgetting, and repressed from consciousness the pathogenic experiences. I called this hypothetical process "repression" (*Verdrängung*), and considered that it was proved by the undeniable existence of resistance.

But now the question arose: what were those forces, and what were the conditions of this repression, in which we were now able to recognize the pathogenic mechanism of hysteria? A comparative study of the pathogenic situations, which the cathartic treatment has made possible, allows us to answer this question. In all those experiences, it had happened that a wish had been aroused, which was in sharp opposition to the other desires of the individual, and was not capable of being reconciled with the ethical, æsthetic and personal pretensions of the patient's personality. There had been a short conflict, and the end of this inner struggle was the repression of the idea which presented itself to consciousness as the bearer of this irreconcilable wish. This was, then, repressed from consciousness and forgotten. The incompatibility of the idea in question with the "ego" of the patient was the motive of the repression, the ethical and other pretensions of the individual were the repressing forces. The presence of the incompatible wish, or the duration of the conflict, had given rise to a high degree of mental pain; this pain was avoided by the repression. This latter process is evidently in such a case a device for the protection of the personality.

I will not multiply examples, but will give you the history of a single one of my cases, in which the conditions and the utility of the repression process stand out clearly enough. Of course for my purpose I must abridge the history of the case and omit many valuable theoretical considerations. It is that of a young girl, who was deeply attached to her father, who had died a short time before, and in whose care she had shared — a situation analogous to that of Breuer's patient. When her older sister married, the girl grew to feel a peculiar sympathy for her new brother-in-law, which easily passed with her for family tenderness. This sister soon fell ill and died, while the patient and her mother were away. The absent ones were hastily recalled, without being told fully of the painful situation. As the girl stood by the bedside of her dead sister, for one short moment there surged up in her mind an idea, which might be framed in these words: "Now he is free and can marry me." We may be sure that this idea, which betrayed to her consciousness her intense love for her brother-

in-law, of which she had not been conscious, was the next moment consigned to repression by her revolted feelings. The girl fell ill with severe hysterical symptoms, and, when I came to treat the case, it appeared that she had entirely forgotten that scene at her sister's bedside and the unnatural, egoistic desire which had arisen in her. She remembered it during the treatment, reproduced the pathogenic moment with every sign of intense emotional excitement, and was cured by this treatment.¹

Perhaps I can make the process of repression and its necessary relation to the resistance of the patient, more concrete by a rough illustration, which I will derive from our present situation.

Suppose that here in this hall and in this audience, whose exemplary stillness and attention I cannot sufficiently commend, there is an individual who is creating a disturbance; and, by his ill-bred laughing, talking, by scraping his feet, distracts my attention from my task. I explain that I cannot go on with my lecture under these conditions, and thereupon several strong men among you get up, and, after a short struggle, eject the disturber of the peace from the hall. He is now "repressed," and I can continue my lecture. But in order that the disturbance may not be repeated, in case the man who has just been thrown out attempts to force his way back into the room, the gentlemen who have executed my suggestion take their chairs to the door and establish themselves there as a "resistance," to keep up the repression. Now, if you transfer both locations to the psyche, calling this "consciousness," and the outside the "unconscious," you have a tolerably good illustration of the process of repression.

We can see now the difference between our theory and that of Janet. We do not derive the psychic fission from a congenital lack of capacity on the part of the mental apparatus to synthesize its experiences, but we explain it dynamically by the conflict of opposing mental forces, we recognize in it the result of an active striving of each mental complex against the other.

New questions at once arise in great number from our theory. The situation of psychic conflict is a very frequent one; an attempt of the ego to defend itself from painful memories can be observed everywhere, and yet the result is not a mental fission. We cannot avoid the assumption that still other conditions are necessary, if the conflict is to result in dissociation. I willingly concede that with the assumption of

¹ This case has been translated by Dr. Brill in "Selected papers on hysteria," etc., p. 31—F 4.

"repression" we stand, not at the end, but at the very beginning of a psychological theory. But we can advance only one step at a time, and the completion of our knowledge must await further and more thorough work.

Now do not attempt to bring the case of Breuer's patient under the point of view of repression. This history cannot be subjected to such an attempt, for it was gained with the help of hypnotic influence. Only when hypnosis is excluded can you see the resistances and repressions and get a correct idea of the pathogenic process. Hypnosis conceals the resistances and so makes a certain part of the mental field freely accessible. By this same process the resistances on the borders of this field are heaped up into a rampart, which makes all beyond inaccessible.

The most valuable things that we have learned from Breuer's observations were his conclusions as to the connection of the symptoms with the pathogenic experiences or psychic traumata, and we must not neglect to evaluate this result properly from the standpoint of the repression-theory. It is not at first evident how we can get from the repression to the creation of the symptoms. Instead of giving a complicated theoretical derivation, I will return at this point to the illustration which I used to typify repression.

Remember that with the ejection of the rowdy and the establishment of the watchers before the door, the affair is not necessarily ended. It may very well happen that the ejected man, now embittered and quite careless of consequences, gives us more to do. He is no longer among us, we are free from his presence, his scornful laugh, his half-audible remarks, but in a certain sense the repression has miscarried, for he makes a terrible uproar outside, and by his outcries and by hammering on the door with his fists interferes with my lecture more than before. Under these circumstances it would be hailed with delight if possibly our honored president, Dr. Stanley Hall, should take upon himself the rôle of peacemaker and mediator. He would speak with the rowdy on the outside, and then turn to us with the recommendation that we let him in again, provided he would guarantee to behave himself better. On Dr. Hall's authority we decide to stop the repression, and now quiet and peace reign again. This is in fact a fairly good presentation of the task devolving upon the physician in the psychoanalytic therapy of neuroses. To say the same thing more directly: we come to the conclusion, from working with hysterical patients and other neurotics, that they have not fully succeeded in repressing the idea to which the incompatible wish is attached. They have, indeed, driven it out of consciousness and out of memory, and apparently saved them-

selves a great amount of psychic pain, *but in the unconscious the suppressed wish still exists*, only waiting for its chance to become active, and finally succeeds in sending into consciousness, instead of the repressed idea, a disguised and unrecognizable surrogate-creation (*Ersatzbildung*), to which the same painful sensations associate themselves that the patient thought he was rid of through his repression. This surrogate of the suppressed idea—the symptom—is secure against further attacks from the defences of the ego, and instead of a short conflict there originates now a permanent suffering. We can observe in the symptom, besides the tokens of its disguise, a remnant of traceable similarity with the originally repressed idea; the way in which the surrogate is built up can be discovered during the psychoanalytic treatment of the patient, and for his cure the symptom must be traced back over the same route to the repressed idea. If this repressed material is once more made part of the conscious mental functions—a process which supposes the overcoming of considerable resistance—the psychic conflict which then arises, the same which the patient wished to avoid, is made capable of a happier termination, under the guidance of the physician, than is offered by repression. There are several possible suitable decisions which can bring conflict and neurosis to a happy end; in particular cases the attempt may be made to combine several of these. Either the personality of the patient may be convinced that he has been wrong in rejecting the pathogenic wish, and he may be made to accept it either wholly or in part; or this wish may itself be directed to a higher goal which is free from objection, by what is called sublimation (*Sublimierung*); or the rejection may be recognized as rightly motivated, and the automatic and therefore insufficient mechanism of repression be reinforced by the higher, more characteristically human mental faculties: one succeeds in mastering his wishes by conscious thought.

Forgive me if I have not been able to present more clearly these main points of the treatment which is to-day known as “psychoanalysis.” The difficulties do not lie merely in the newness of the subject.

Regarding the nature of the unacceptable wishes, which succeed in making their influence felt out of the unconscious, in spite of repression; and regarding the question of what subjective and constitutional factors must be present for such a failure of repression and such a surrogate or symptom creation to take place, we will speak in later remarks.

THIRD LECTURE

Ladies and Gentlemen: It is not always easy to tell the

truth, especially when one must be brief, and so to-day I must correct an incorrect statement that I made in my last lecture.

I told you how when I gave up using hypnosis I pressed my patients to tell me what came into their minds that had to do with the problem we were working on, I told them that they would remember what they had apparently forgotten, and that the thought which irrupted into consciousness (*Einfall*) would surely embody the memory for which we were seeking. I claimed that I substantiated the fact that the first idea of my patients brought the right clue and could be shown to be the forgotten continuation of the memory. Now this is not always so; I represented it as being so simple only for purposes of abbreviation. In fact, it would only happen the first times that the right forgotten material would emerge through simple pressure on my part. If the experience was continued, ideas emerged in every case which could not be the right ones, for they were not to the purpose, and the patients themselves rejected them as incorrect. Pressure was of no further service here, and one could only regret again having given up hypnosis. In this state of perplexity I clung to a prejudice which years later was proved by my friend C. G. Jung of the University of Zürich, and his pupils to have a scientific justification. I must confess that it is often of great advantage to have prejudices. I put a high value on the strength of the determination of mental processes, and I could not believe that any idea which occurred to the patient, which originated in a state of concentrated attention, could be quite arbitrary and out of all relation to the forgotten idea that we were seeking. That it was not identical with the latter, could be satisfactorily explained by the hypothetical psychological situation. In the patients whom I treated there were two opposing forces: on the one hand the conscious striving to drag up into consciousness the forgotten experience which was present in the unconscious; and on the other hand the resistance which we have seen, which set itself against the emergence of the suppressed idea or its associates into consciousness. In case this resistance was non-existent or very slight, the forgotten material could become conscious without disguise (*Enstellung*). It was then a natural supposition that the disguise would be the more complete, the greater the resistance to the emergence of the idea. Thoughts which broke into the patient's consciousness instead of the ideas sought for, were accordingly made up just like symptoms; they were new, artificial, ephemeral surrogates for the repressed ideas, and differed from these just in proportion as they had been more completely disguised under the influence of the resistances. These surrogates must, however, show a certain similarity with the ideas which are the object of our search, by

virtue of their nature as symptoms; and when the resistance is not too intensive it is possible from the nature of these irruptions to discover the hidden object of our search. This must be related to the repressed thought as a sort of allusion, as a statement of the same thing in *indirect* terms.

We know cases in normal psychology in which analogous situations to the one which we have assumed give rise to similar experiences. Such a case is that of wit. By my study of psychoanalytic technique I was necessarily led to a consideration of the problem of the nature of wit. I will give one example of this sort, which, too, is a story that originally appeared in English.

The anecdote runs: ¹Two unscrupulous business men had succeeded by fortunate speculations in accumulating a large fortune, and then directed their efforts to breaking into good society. Among other means they thought it would be of advantage to be painted by the most famous and expensive artist of the city, a man whose paintings were considered as events. The costly paintings were first shown at a great soirée and both hosts led the most influential connoisseur and art critic to the wall of the salon on which the portraits were hung, to elicit his admiring judgment. The artist looked for a long time, looked about as though in search of something, and then merely asked, pointing out the vacant space between the two pictures; "And where is the Saviour?"

I see that you are all laughing over this good example of wit, which we will now attempt to analyse. We understand that the critic means to say; "You are a couple of malefactors, like those between whom the Saviour was crucified." But he does not say this, he expresses himself instead in a way that at first seems not to the purpose and not related to the matter in hand, but which at the next moment we recognize as an *allusion* to the insult at which he aims, and as a perfect surrogate for it. We cannot expect to find in the case of wit all those relations that our theory supposes for the origin of the irruptive ideas of our patients, but it is my desire to lay stress on the similar motivation of wit and irruptive idea. Why does not the critic say directly what he has to say to the two rogues? Because, in addition to his desire to say it straight out, he is actuated by strong opposite motives. It is a proceeding which is liable to be dangerous to offend people who are one's hosts, and who can call to their aid the strong arms of numerous servants. One might easily suffer the same fate that I used in the previous lecture to illustrate repression. On this ground,

¹Der Witz und seine Beziehung zum Unbewussten. Deuticke, Vienna, 1905, p. 59.

the critic does not express the particular insult directly, but in a disguised form, as an allusion with omission. The same constellation comes into play, according to our hypothesis, when our patient produces the irruptive idea as a surrogate for the forgotten idea which is the object of the quest.

Ladies and gentlemen, it is very useful to designate a group of ideas which belong together and have a common emotive tone, according to the custom of the Zürich school (Bleuler, Jung and others), as a "complex." So we can say that if we set out from the last memories of the patient to look for a repressed complex, that we have every prospect of discovering it, if only the patient will communicate to us a sufficient number of the ideas which come into his head. So we let the patient speak along any line that he desires, and cling to the hypothesis that nothing can occur to him except what has some indirect bearing on the complex that we are seeking. If this method of discovering the repressed complexes seems too circumstantial, I can at least assure you that it is the only available one.

In practicing this technique, one is further bothered by the fact that the patient often stops, is at a stand-still, and considers that he has nothing to say; nothing occurs to him. If this were really the case and the patient were right, our procedure would again be proven inapplicable. Closer observation shows that such an absence of ideas never really occurs, and that it only appears to when the patient holds back or rejects the idea which he perceives, under the influence of the resistance, which disguises itself as critical judgment of the value of the idea. The patient can be protected from this if he is warned in advance of this circumstance, and told to take no account of the critical attitude. He must say anything that comes into his mind, fully laying aside such critical choice, even though he may think it unessential, irrelevant, nonsensical, especially when the idea is one which is unpleasant to dwell on. By following this prescription we secure the material which sets us on the track of the repressed complex.

These irruptive ideas, which the patient himself values little, if he is under the influence of the resistance and not that of the physician, are for the psychologist like the ore, which by simple methods of interpretation he reduces from its crude state to valuable metal. If one desires to gain in a short time a preliminary knowledge of the patient's repressed complexes, without going into the question of their arrangement and associations, this examination may be conducted with the help of the association experiments, as Jung¹ and his pupils have per-

¹C. G. Jung: Diagnostische Assoziationsstudien, B. 1, 1906.

fected them. This procedure is to the psychologist what qualitative analysis is to the chemist; it may be dispensed with in the therapy of neurotic patients, but is indispensable in the investigations of the psychoses, which have been begun by the Zürich school with such valuable results.

This method of work with whatever comes into the patient's head when he submits to psychoanalytic treatment, is not the only technical means at our disposal for the widening of consciousness. Two other methods of procedure serve the same purpose, the interpretation of his dreams and the evaluation of acts which he bungles or does without intending to (*Fehl- und Zufallshandlungen*).

I might say, esteemed hearers, that for a long time I hesitated whether instead of this hurried survey of the whole field of psychoanalysis, I should not rather offer you a thorough consideration of the analysis of dreams; a purely subjective and apparently secondary motive decided me against this. It seemed rather an impropriety that in this country, so devoted to practical pursuits, I should pose as "interpreter of dreams," before you had a chance to discover what significance the old and despised art can claim.

Interpretation of dreams is in fact the *via regia* to the interpretation of the unconscious, the surest ground of psychoanalysis and a field in which every worker must win his convictions and gain his education. If I were asked how one could become a psychoanalyst, I should answer, through the study of his own dreams. With great tact all opponents of the psychoanalytic theory have so far either evaded any criticism of the "*Traumdeutung*"¹ or have attempted to pass over it with the most superficial objections. If, on the contrary, you will undertake the solution of the problems of dream life, the novelties which psychoanalysis present to your thoughts will no longer be difficulties.

You must remember that our nightly dream productions show the greatest outer similarity and inner relationship to the creations of the insane, but on the other hand are compatible with full health during waking life. It does not sound at all absurd to say that whoever regards these normal sense illusions, these delusions and alterations of character as matter for amazement instead of understanding, has not the least prospect of understanding the abnormal creations of diseased mental states in any other than the lay sense. You may with confidence place in this lay group all the psychiatrists of to-day. Follow me now on a brief excursion through the field of dream problems.

¹Die Traumdeutung: 2d edition. Deuticke, Vienna, 1909.

In our waking state we usually treat dreams with as little consideration as the patient treats the irruptive ideas which the psychoanalyst demands from him. It is evident that we reject them, for we forget them quickly and completely. The slight valuation which we place on them is based, with those dreams that are not confused and nonsensical, on the feeling that they are foreign to our personality, and, with other dreams, on their evident absurdity and senselessness. Our rejection derives support from the unrestrained shamelessness and the immoral longings which are obvious in many dreams. Antiquity, as we know, did not share this light valuation of dreams. The lower classes of our people to-day stick close to the value which they set on dreams; they, however, expect from them, as did the ancients, the revelation of the future. I confess that I see no need to adopt mystical hypotheses to fill out the gaps in our present knowledge, and so I have never been able to find anything that supported the hypothesis of the prophetic nature of dreams. Many other things, which are wonderful enough, can be said about them.

And first, not all dreams are so foreign to the character of the dreamer, are incomprehensible and confused. If you will undertake to consider the dreams of young children from the age of a year and a half on, you will find them quite simple and easy to interpret. The young child always dreams of the fulfillment of wishes which were aroused in him the day before and were not satisfied. You need no art of interpretation to discover this simple solution, you only need to inquire into the experiences of the child on the day before (the "dream day"). Now it would certainly be a most satisfactory solution of the dream-riddle, if the dreams of adults, too, were the same as those of children, fulfillments of wishes which had been aroused in them during the dream day. This is actually the fact; the difficulties which stand in the way of this solution can be removed step by step by a thorough analysis of the dream.

There is, first of all, the most weighty objection, that the dreams of adults generally have an incomprehensible content, which shows wish-fulfillment least of anything. The answer is this: these dreams have undergone a process of disguise, the psychic content which underlies them was originally meant for quite different verbal expression. You must differentiate between the *manifest dream-content*, which we remember in the morning only confusedly, and with difficulty clothe in words which seem arbitrary, and the *latent dream-thoughts*, whose presence in the unconscious we must assume. This distortion of the dream (*Traumentstellung*) is the same process which has been revealed to you in the investigations of the creations

(*symptoms*) of hysterical subjects; it points to the fact that the same opposition of psychic forces has its share in the creation of dreams as in the creation of symptoms.

The manifest dream-content is the disguised surrogate for the unconscious dream thoughts, and this disguising is the work of the defensive forces of the ego, of the resistances. These prevent the repressed wishes from entering consciousness during the waking life, and even in the relaxation of sleep they are still strong enough to force them to hide themselves by a sort of masquerading. The dreamer, then, knows just as little the sense of his dream as the hysterical knows the relation and significance of his symptoms. That there are latent dream-thoughts and that between them and the manifest dream-content there exists the relation just described—of this you may convince yourselves by the analysis of dreams, a procedure the technique of which is exactly that of psychoanalysis. You must abstract entirely from the apparent connection of the elements in the manifest dream and seek for the irruptive ideas which arise through free association, according to the psychoanalytic laws, from each separate dream element. From this material the latent dream thoughts may be discovered, exactly as one divines the concealed complexes of the patient from the fancies connected with his symptoms and memories. From the latent dream thoughts which you will find in this way, you will see at once how thoroughly justified one is in interpreting the dreams of adults by the same rubrics as those of children. What is now substituted for the manifest dream-content is the real sense of the dream, is always clearly comprehensible, associated with the impressions of the day before, and appears as the fulfilling of an unsatisfied wish. The manifest dream, which we remember after waking, may then be described as a *disguised* fulfillment of *repressed* wishes.

It is also possible by a sort of synthesis to get some insight into the process which has brought about the disguise of the unconscious dream thoughts as the manifest dream-content. We call this process "dream-work" (*Traumarbeit*). This deserves our fullest theoretical interest, since here as nowhere else we can study the unsuspected psychic processes which are existent in the unconscious, or, to express it more exactly, *between* two such separate systems as the conscious and the unconscious. Among these newly discovered psychic processes, two, condensation (*Verdichtung*) and displacement or transvaluation, change of psychic accent (*Verschiebung*), stand out most prominently. Dream work is a special case of the reaction of different mental groupings on each other, and as such is the consequence of psychic fission. In all essential points it seems identical with the work of disguise, which

changes the repressed complex in the case of failing repression into symptoms.

You will furthermore discover by the analysis of dreams, most convincingly your own, the unsuspected importance of the rôle which impressions and experiences from early childhood exert on the development of men. In the dream life the child, as it were, continues his existence in the man, with a retention of all his traits and wishes, including those which he was obliged to allow to fall into disuse in his later years. With irresistible might it will be impressed on you by what processes of development, of repression, sublimation and reaction there arises out of the child, with its peculiar gifts and tendencies, the so-called normal man, the bearer and partly the victim of our painfully acquired civilization. I will also direct your attention to the fact that we have discovered from the analysis of dreams that the unconscious makes use of a sort of symbolism, especially in the presentation of sexual complexes. This symbolism in part varies with the individual, but in part is of a typical nature, and seems to be identical with the symbolism which we suppose to lie behind our myths and legends. It is not impossible that these latter creations of the people may find their explanation from the study of dreams.

Finally, I must remind you that you must not be led astray by the objection that the occurrence of anxiety-dreams (*Angstträume*), contradicts our idea of the dream as a wish-fulfillment. Apart from the consideration that anxiety-dreams also require interpretation before judgment can be passed on them, one can say quite generally that the anxiety does not depend in such a simple way on the dream content as one might suppose without more knowledge of the facts, and more attention to the conditions of neurotic anxiety. Anxiety is one of the ways in which the ego relieves itself of repressed wishes which have become too strong, and so is easy to explain in the dream, if the dream has gone too far towards the fulfilling of the objectionable wish.

You see that the investigation of dreams was justified by the conclusions which it has given us concerning things otherwise hard to understand. But we came to it in connection with the psychoanalytic treatment of neurotics. From what has been said you can easily understand how the interpretation of dreams, if it is not made too difficult by the resistance of the patient, can lead to a knowledge of the patient's concealed and repressed wishes and the complexes which he is nourishing. I may now pass to that group of everyday mental phenomena whose study has become a technical help for psychoanalysis.

These are the bungling of acts (*Fehlhandlungen*) among

normal men as well as among neurotics, to which no significance is ordinarily attached; the forgetting of things which one is supposed to know and at other times really does know (for example the temporary forgetting of proper names); mistakes in speaking (*Versprechen*), which occur so frequently; analogous mistakes in writing (*Verschreiben*) and in reading (*Verlesen*), the automatic execution of purposive acts in wrong situations (*Vergreifen*) and the loss or breaking of objects, etc. These are trifles, for which no one has ever sought a psychological determination, which have passed unchallenged as chance experiences, as consequences of absent-mindedness, inattention and similar conditions. Here, too, are included the acts and gestures executed without being noticed by the subject, to say nothing of the fact that he attaches no psychic importance to them; as playing and trifling with objects, humming melodies, handling one's person and clothing and the like.¹

These little things, the bungling of acts, like the symptomatic and chance acts (*Symptom- und Zufallshandlungen*) are not so entirely without meaning as is generally supposed by a sort of tacit agreement. They have a meaning, generally easy and sure to interpret from the situation in which they occur, and it can be demonstrated that they either express impulses and purposes which are repressed, hidden if possible from the consciousness of the individual, or that they spring from exactly the same sort of repressed wishes and complexes which we have learned to know already as the creators of symptoms and dreams.

It follows that they deserve the rank of symptoms, and their observation, like that of dreams, can lead to the discovery of the hidden complexes of the psychic life. With their help one will usually betray the most intimate of his secrets. If these occur so easily and commonly among people in health, with whom repression has on the whole succeeded fairly well, this is due to their insignificance and their inconspicuous nature. But they can lay claim to high theoretic value, for they prove the existence of repression and surrogate creations even under the conditions of health. You have already noticed that the psychoanalyst is distinguished by an especially strong belief in the determination of the psychic life. For him there is in the expressions of the psyche nothing trifling, nothing arbitrary and lawless, he expects everywhere a widespread motivation, where customarily such claims are not made; more than that, he is even prepared to find a manifold motivation of these

¹Zur Psychopathologie des Alltagslebens. 3d edition, 1910. S. Karger, Berlin.

psychic expressions, while our supposedly inborn causal need is satisfied with a single psychic cause.

Now keeping in mind the means which we possess for the discovery of the hidden, forgotten, repressed things in the soul life: the study of the irruptive ideas called up by free association, the patient's dreams, and his bungled and symptomatic acts; and adding to these the evaluation of other phenomena which emerge during the psychoanalytic treatment, on which I shall later make a few remarks under the heading of "transfer" (*Uebertragung*), you will come with me to the conclusion that our technique is already sufficiently efficacious for the solution of the problem of how to introduce the pathogenic psychic material into consciousness, and so to do away with the suffering brought on by the creation of surrogate symptoms.

The fact that by such therapeutic endeavors our knowledge of the mental life of the normal and the abnormal is widened and deepened, can of course only be regarded as an especial attraction and superiority of this method.

I do not know whether you have gained the impression that the technique through whose arsenal I have led you is a peculiarly difficult one. I consider that on the contrary, for one who has mastered it, it is quite adapted for use. But so much is sure, that it is not obvious, that it must be learned no less than the histological or the surgical technique.

You may be surprised to learn that in Europe we have heard very frequently judgments passed on psychoanalysis by persons who knew nothing of its technique and had never practised it, but who demanded scornfully that we show the correctness of our results. There are among these people some who are not in other things unacquainted with scientific methods of thought, who for example would not reject the result of a microscopical research because it cannot be confirmed with the naked eye in anatomical preparations, and who would not pass judgment until they had used the microscope. But in matters of psychoanalysis circumstances are really more unfavorable for gaining recognition. Psychoanalysis will bring the repressed in mental life to conscious acknowledgment, and every one who judges it is himself a man who has such repressions, perhaps only maintained with difficulty. It will consequently call forth the same resistances from him as from the patient, and this resistance can easily succeed in disguising itself as intellectual rejection, and bring forward arguments similar to those from which we protect our patients by the basic principles of psychoanalysis. It is not difficult to substantiate in our opponents the same impairment of intelligence produced by emotivity which we may observe every day with our patients.

The arrogance of consciousness which for example rejects dreams so lightly, belongs—quite generally—to the strongest protective apparatus which guards us against the breaking through of the unconscious complexes, and as a result it is hard to convince people of the reality of the unconscious, and to teach them anew, what their conscious knowledge contradicts.

FOURTH LECTURE

Ladies and Gentlemen: At this point you will be asking what the technique which I have described has taught us of the nature of the pathogenic complexes and repressed wishes of neurotics.

One thing in particular: psychoanalytic investigations trace back the symptoms of disease with really surprising regularity to impressions from the sexual life, show us that the pathogenic wishes are of the nature of erotic impulse-components (*Triebkomponente*), and necessitate the assumption that to disturbances of the erotic sphere must be ascribed the greatest significance among the etiological factors of the disease. This holds of both sexes.

I know that this assertion will not willingly be credited. Even those investigators who gladly follow my psychological labors, are inclined to think that I overestimate the etiological share of the sexual moments. They ask me why other mental excitations should not lead to the phenomena of repression and surrogate-creation which I have described. I can give them this answer; that I do not know why they should not do this, I have no objection to their doing it, but experience shows that they do not possess such a significance, and that they merely support the effect of the sexual moments, without being able to supplant them. This conclusion was not a theoretical postulate; in the *Studien über Hysterie*, published in 1895 with Dr. Breuer, I did not stand on this ground. I was converted to it when my experience was richer and had led me deeper into the nature of the case. Gentlemen, there are among you some of my closest friends and adherents, who have travelled to Worcester with me. Ask them, and they will tell you that they all were at first completely sceptical of the assertion of the determinative significance of the sexual etiology, until they were compelled by their own analytic labors to come to the same conclusion.

The conduct of the patients does not make it any easier to convince one's self of the correctness of the view which I have expressed. Instead of willingly giving us information concerning their sexual life, they try to conceal it by every means in their power. Men generally are not candid in sexual mat-

ters. They do not show their sexuality freely, but they wear a thick overcoat—a fabric of lies—to conceal it, as though it were bad weather in the world of sex. And they are not wrong; sun and wind are not favorable in our civilized society to any demonstration of sex life. In truth no one can freely disclose his erotic life to his neighbor. But when your patients see that in your treatment they may disregard the conventional restraints, they lay aside this veil of lies, and then only are you in a position to formulate a judgment on the question in dispute. Unfortunately physicians are not favored above the rest of the children of men in their personal relationship to the questions of the sex life. Many of them are under the ban of that mixture of prudery and lasciviousness which determines the behaviour of most *Kulturmenschen* in affairs of sex.

Now to proceed with the communication of our results. It is true that in another series of cases psychoanalysis at first traces the symptoms back not to the sexual, but to banal traumatic experiences. But the distinction loses its significance through other circumstances. The work of analysis which is necessary for the thorough explanation and complete cure of a case of sickness does not stop in any case with the experience of the time of onset of the disease, but in every case it goes back to the adolescence and the early childhood of the patient. Here only do we hit upon the impressions and circumstances which determine the later sickness. Only the childhood experiences can give the explanation for the sensitivity to later traumata and only when these memory traces, which almost always are forgotten, are discovered and made conscious, is the power developed to banish the symptoms. We arrive here at the same conclusion as in the investigation of dreams—that it is the incompatible, repressed wishes of childhood which lend their power to the creation of symptoms. Without these the reactions upon later traumata discharge normally. But we must consider these mighty wishes of childhood very generally as sexual in nature.

Now I can at any rate be sure of your astonishment. Is there an infantile sexuality? you will ask. Is childhood not rather that period of life which is distinguished by the lack of the sexual impulse? No, gentlemen, it is not at all true that the sexual impulse enters into the child at puberty, as the devils in the gospel entered into the swine. The child has his sexual impulses and activities from the beginning, he brings them with him into the world, and from these the so-called normal sexuality of adults emerges by a significant development through manifold stages. It is not very difficult to observe the expressions of this childish sexual activity; it needs

rather a certain art to overlook them or to fail to interpret them.¹

As fate would have it, I am in a position to call a witness for my assertions from your own midst. I show you here the work of one Dr. Sanford Bell, published in 1902 in the *American Journal of Psychology*. The author was a fellow of Clark University, the same institution within whose walls we now stand. In this thesis, entitled "A Preliminary Study of the Emotion of Love between the Sexes," which appeared three years before my "Drei Abhandlungen zur Sexualtheorie," the author says just what I have been saying to you: "The emotion of sex love . . . does not make its appearance for the first time at the period of adolescence as has been thought." He has, as we should say in Europe, worked by the American method, and has gathered not less than 2,500 positive observations in the course of fifteen years, among them 800 of his own. He says of the signs by which this amorous condition manifests itself: "The unprejudiced mind, in observing these manifestations in hundreds of couples of children, cannot escape referring them to sex origin. The most exacting mind is satisfied when to these observations are added the confessions of those who have as children experienced the emotion to a marked degree of intensity, and whose memories of childhood are relatively distinct." Those of you who are unwilling to believe in infantile sexuality will be most astonished to hear that among those children who fell in love so early not a few are of the tender ages of three, four, and five years.

It would not be surprising if you should believe the observations of a fellow-countryman rather than my own. Fortunately a short time ago from the analysis of a five-year-old boy who was suffering from anxiety, an analysis undertaken with correct technique by his own father,² I succeeded in getting a fairly complete picture of the bodily expressions of the impulse and the mental productions of an early stage of childish sexual life. And I must remind you that my friend, Dr. C. G. Jung, read you a few hours ago in this room an observation on a still younger girl who from the same cause as my patient—the birth of a little child in the family—betrayed certainly almost the same secret excitement, wish and complex-creation. Accordingly I am not without hope that you may feel friendly toward this idea of infantile sexuality that was so strange at first. I might also quote the remarkable example of the Zürich psychiatrist, E. Bleuler, who said a few years

¹ Drei Abhandlungen zur Sexualtheorie. Wien, F. Deuticke, 1908' 2d ed.

² Analyse der Phobie eines 5-jährigen Knaben. Jahrbuch f. Psychoanalytische u. psychopathologische Forschungen. B. I, H. I., 1909.

ago openly that he faced my sexual theories incredulous and bewildered, and since that time by his own observations had substantiated them in their whole scope.¹ If it is true that most men, medical observers and others, do not want to know anything about the sexual life of the child, the fact is capable of explanation only too easily. They have forgotten their own infantile sexual activity under the pressure of education for civilization and do not care to be reminded now of the repressed material. You will be convinced otherwise if you begin the investigation by a self-analysis, by an interpretation of your own childhood memories.

Lay aside your doubts and let us evaluate the infantile sexuality of the earliest years.² The sexual impulse of the child manifests itself as a very complex one, it permits of an analysis into many components, which spring from different sources. It is entirely disconnected from the function of reproduction which it is later to serve. It permits the child to gain different sorts of pleasure sensations, which we include, by the analogues and connections which they show, under the term sexual pleasures. The great source of infantile sexual pleasure is the auto-excitation of certain particularly sensitive parts of the body; besides the genitals are included the rectum and the opening of the urinary canal, and also the skin and other sensory surfaces. Since in this first phase of child sexual life the satisfaction is found on the child's own body and has nothing to do with any other object, we call this phase after a word coined by Havelock Ellis, that of "auto-erotism." The parts of the body significant in giving sexual pleasure we call "erogenous zones." The thumb-sucking (*Ludeln*) or passionate sucking (*Wonnesaugen*) of very young children is a good example of such an auto-erotic satisfaction of an erogenous zone. The first scientific observer of this phenomenon, a specialist in children's diseases in Budapest by the name of Lindner, interpreted these rightly as sexual satisfaction and described exhaustively their transformation into other and higher forms of sexual gratification.³ Another sexual satisfaction of this time of life is the excitation of the genitals by masturbation, which has such a great significance for later life and, in the case of many individuals, is never fully overcome. Besides this and other auto-erotic manifestations we see very early in the child the impulse-components of *sexual pleasure*, or, as we may say, of the *libido*, which presupposes a second person as its object. These impulses appear in opposed pairs,

¹ Bleuler: Sexuelle Abnormitäten der Kinder. Jahrbuch der schweizer, Gesellschaft für Schulgesundheitspflege. IX, 1908.

² Drei Abhandlungen zur Sexualtheorie, Vienna, 1910, 2d ed.

³ Jahrbuch f. Kinderheilkunde, 1879.

as active and passive. The most important representatives of this group are the pleasure in inflicting pain (sadism) with its passive opposite (masochism) and active and passive exhibition-pleasure (*Schaulust*). From the first of these later pairs splits off the curiosity for knowledge, as from the latter the impulse toward artistic and theatrical representation. Other sexual manifestations of the child can already be regarded from the view-point of object-choice, in which the second person plays the prominent part. The significance of this was primarily based upon motives of the impulse of self-preservation. The difference between the sexes plays, however, in the child no very great rôle. One may attribute to every child, without wronging him, a bit of the homosexual disposition.

The sexual life of the child, rich, but dissociated, in which each single impulse goes about the business of arousing pleasure independently of every other, is later correlated and organized in two general directions, so that by the close of puberty the definite sexual character of the individual is practically finally determined. The single impulses subordinate themselves to the overlordship of the genital zone, so that the whole sexual life is taken over into the service of procreation, and their gratification is now significant only so far as they help to prepare and promote the true sexual act. On the other hand, object-choice prevails over auto-erotism, so that now in the sexual life all components of the sexual impulse are satisfied in the loved person. But not all the original impulse-components are given a share in the final shaping of the sexual life. Even before the advent of puberty certain impulses have undergone the most energetic repression under the impulse of education, and mental forces like shame, disgust and morality are developed, which, like sentinels, keep the repressed wishes in subjection. When there comes, in puberty, the high tide of sexual desire it finds dams in this creation of reactions and resistances. These guide the outflow into the so-called normal channels, and make it impossible to revivify the impulses which have undergone repression.

The most important of these repressed impulses are koprophilism, that is, the pleasure in children connected with the excrements; and, further, the tendencies attaching themselves to the persons of the primitive object-choice.

Gentlemen, a sentence of general pathology says that every process of development brings with it the germ of pathological dispositions in so far as it may be inhibited, delayed, or incompletely carried out. This holds for the development of the sexual function, with its many complications. It is not smoothly completed in all individuals, and may leave behind either abnormalities or disposition to later diseases by the way

of later falling back or *regression*. It may happen that not all the partial impulses subordinate themselves to the rule of the genital zone. Such an impulse which has remained disconnected brings about what we call a perversion, which may replace the normal sexual goal by one of its own. It may happen, as has been said before, that the auto-erotism is not fully overcome, as many sorts of disturbances testify. The originally equal value of both sexes as sexual objects may be maintained and an inclination to homosexual activities in adult life result from this, which, under suitable conditions, rises to the level of exclusive homosexuality. This series of disturbances corresponds to the direct inhibition of development of the sexual function, it includes the perversions and the general *infantilism* of the sex life that are not seldom met with.

The disposition to neuroses is to be derived in another way from an injury to the development of the sex life. The neuroses are related to the perversions as the negative to the positive; in them we find the same impulse-components as in perversions, as bearers of the complexes and as creators of the symptoms; but here they work from out the unconscious. They have undergone a repression, but in spite of this they maintain themselves in the unconscious. Psychoanalysis teaches us that overstrong expression of the impulse in very early life leads to a sort of fixation (*Fixierung*), which then offers a weak point in the articulation of the sexual function. If the exercise of the normal sexual function meets with hindrances in later life, this repression, dating from the time of development, is broken through at just that point at which the infantile fixation took place.

You will now perhaps make the objection: "But all that is not sexuality." I have used the word in a very much wider sense than you are accustomed to understand it. This I willingly concede. But it is a question whether you do not rather use the word in much too narrow a sense when you restrict it to the realm of procreation. You sacrifice by that the understanding of perversions; of the connection between perversion, neurosis and normal sexual life; and have no means of recognizing, in its true significance, the easily observable beginning of the somatic and mental sexual life of the child. But however you decide about the use of the word, remember that the psychoanalyst understands sexuality in that full sense to which he is led by the evaluation of infantile sexuality.

Now we turn again to the sexual development of the child. We still have much to say here, since we have given more attention to the somatic than to the mental expressions of the sexual life. The primitive object-choice of the child, which is derived from his need of help, demands our further interest.

It first attaches to all persons to whom he is accustomed, but soon these give way in favor of his parents. The relation of the child to his parents is, as both direct observation of the child and later analytic investigation of adults agree, not at all free from elements of sexual accessory-excitation (*Miterregung*). The child takes both parents, and especially one, as an object of his erotic wishes. Usually he follows in this the stimulus given by his parents, whose tenderness has very clearly the character of a sex manifestation, though inhibited so far as its goal is concerned. As a rule, the father prefers the daughter, the mother the son; the child reacts to this situation, since, as son, he wishes himself in the place of his father, as daughter, in the place of the mother. The feelings awakened in these relations between parents and children, and, as a resultant of them, those among the children in relation to each other, are not only positively of a tender, but negatively of an inimical sort. The complex built up in this way is destined to quick repression, but it still exerts a great and lasting effect from the unconscious. We must express the opinion that this with its ramifications presents the *nuclear complex* of every neurosis, and so we are prepared to meet with it in a not less effectual way in the other fields of mental life. The myth of King Œdipus, who kills his father and wins his mother as a wife is only the slightly altered presentation of the infantile wish, rejected later by the opposing barriers of incest. Shakespeare's tale of Hamlet rests on the same basis of an incest complex, though better concealed. At the time when the child is still ruled by the still unrepressed nuclear complex, there begins a very significant part of his mental activity which serves sexual interest. He begins to investigate the question of where children come from and guesses more than adults imagine of the true relations by deduction from the signs which he sees. Usually his interest in this investigation is awakened by the threat to his welfare through the birth of another child in the family, in whom at first he sees only a rival. Under the influence of the partial impulses which are active in him he arrives at a number of "infantile sexual theories," as that the same male genitals belong to both sexes, that children are conceived by eating and born through the opening of the intestine, and that sexual intercourse is to be regarded as an inimical act, a sort of overpowering.

But just the unfinished nature of his sexual constitution and the gaps in his knowledge brought about by the hidden condition of the feminine sexual canal, cause the infant investigator to discontinue his work as a failure. The facts of this childish investigation itself as well as the infant sex theories created by

it are of determinative significance in the building of the child's character, and in the content of his later neuroses.

It is unavoidable and quite normal that the child should make his parents the objects of his first object-choice. But his *libido* must not remain fixed on these first chosen objects, but must take them merely as a prototype and transfer from these to other persons in the time of definite object-choice. The breaking loose (*Ablösung*) of the child from his parents is thus a problem impossible to escape if the social virtue of the young individual is not to be impaired. During the time that the repressive activity is making its choice among the partial sexual impulses and later, when the influence of the parents, which in the most essential way has furnished the material for these repressions, is lessened, great problems fall to the work of education, which at present certainly does not always solve them in the most intelligent and economic way.

Gentlemen, do not think that with these explanations of the sexual life and the sexual development of the child we have too far departed from psychoanalysis and the cure of neurotic disturbances. If you like, you may regard the psychoanalytic treatment only as a continued education for the overcoming of childhood-remnants (*Kindheitsresten*).

FIFTH LECTURE

Ladies and Gentlemen: With the discovery of infantile sexuality and the tracing back of the neurotic symptoms to erotic impulse-components we have arrived at several unexpected formulæ for expressing the nature and tendencies of neurotic diseases. We see that the individual falls ill when in consequence of outer hindrances or inner lack of adaptability the satisfaction of the erotic needs in the sphere of reality is denied. We see that he then flees to sickness, in order to find with its help a surrogate satisfaction for that denied him. We recognize that the symptoms of illness contain fractions of the sexual activity of the individual, or his whole sexual life, and we find in the turning away from reality the chief tendency and also the chief injury of the sickness. We may guess that the resistance of our patients against the cure is not a simple one, but is composed of many motives. Not only does the ego of the patient strive against the giving up of the repressions by which it has changed itself from its original constitution into its present form, but also the sexual impulses may not renounce their surrogate satisfaction so long as it is not certain that they can be offered anything better in the sphere of reality.

The flight from the unsatisfying reality into what we call,

on account of its biologically injurious nature, disease, but which is never without an individual gain in pleasure for the patient, takes place over the path of regression, the return to earlier phases of the sexual life, when satisfaction was not lacking. This regression is seemingly a twofold one, a *temporal*, in so far as the *libido* or erotic need falls back to a temporally earlier stage of development, and a *formal*, since the original and primitive psychic means of expression are applied to the expression of this need. Both sorts of regression focus in childhood and have their common point in the production of an infantile condition of sexual life.

The deeper you penetrate into the pathogenesis of neurotic diseases, the more the connection of neuroses with other products of human mentality, even the most valuable, will be revealed to you. You will be reminded that we men, with the high claims of our civilization and under the pressure of our repressions, find reality generally quite unsatisfactory and so keep up a life of fancy in which we love to compensate for what is lacking in the sphere of reality by the production of wish-fulfillments. In these phantasies is often contained very much of the particular constitutional essence of personality and of its tendencies, repressed in real life. The energetic and successful man is he who succeeds by dint of labor in transforming his wish fancies into reality. Where this is not successful in consequence of the resistance of the outer world and the weakness of the individual, there begins the turning away from reality. The individual takes refuge in his satisfying world of fancy. Under certain favorable conditions it still remains possible for him to find another connecting link between these fancies and reality, instead of permanently becoming a stranger to it through the regression into the infantile. If the individual who is displeased with reality is in possession of that *artistic talent* which is still a psychological riddle, he can transform his fancies into artistic creations. So he escapes the fate of a neurosis and wins back his connection with reality by this round-about way.¹ Where this opposition to the real world exists, but this valuable talent fails or proves insufficient, it is unavoidable that the *libido*, following the origin of the fancies, succeeds by means of regression in revivifying the infantile wishes and so producing a neurosis. The neurosis takes, in our time, the place of the cloister, in which were accustomed to take refuge all those whom life had undeceived or who felt themselves too weak for life. Let me give at this point the main result at which we have arrived by the psycho-

¹ Compare, Rank, Otto: *Der Künstler, Ansätze zu einer Sexual-Psychologie*. 56 p. Heller & Co., Wien, 1907.

analytic investigation of neurotics, namely, that neuroses have no peculiar psychic content of their own, which is not also to be found in healthy states; or, as C. G. Jung has expressed it, neurotics fall ill of the same complexes with which we sound people struggle. It depends on quantitative relationships, on the relations of the forces wrestling with each other, whether the struggle leads to health, to a neurosis, or to compensatory over-functioning (*Ueberleistung*).

Ladies and gentlemen, I have still withheld from you the most remarkable experience which corroborates our assumptions of the sexual impulse-forces of neurotics. Every time that we treat a neurotic psychoanalytically, there occurs in him the so-called phenomenon of *transfer* (*Uebertragung*), that is, he applies to the person of the physician a great amount of tender emotion, often mixed with enmity, which has no foundation in any real relation, and must be derived in every respect from the old wish-fancies of the patient which have become unconscious. Every fragment of his emotive life, which can no longer be called back into memory, is accordingly lived over by the patient in his relations to the physician, and only by such a living of them over in the "transfer" is he convinced of the existence and the power of these unconscious sexual excitations. The symptoms, which, to use a simile from chemistry, are the precipitates of earlier love experiences (in the widest sense), can only be dissolved in the higher temperature of the experience of transfer and transformed into other psychic products. The physician plays in this reaction, to use an excellent expression of S. Ferenczi,¹ the rôle of a *catalytic ferment*, which temporarily attracts to itself the affect which has become free in the course of the process.

The study of transfer can also give you the key to the understanding of hypnotic suggestion, which we at first used with our patients as a technical means of investigation of the unconscious. Hypnosis showed itself at that time to be a therapeutic help, but a hindrance to the scientific knowledge of the real nature of the case, since it cleared away the psychic resistances from a certain field, only to pile them up in an unscalable wall at the boundaries of this field. You must not think that the phenomenon of transfer, about which I can unfortunately say only too little here, is created by the influence of the psychoanalytic treatment. The transfer arises spontaneously in all human relations and in the relations of the patient to the physician; it is everywhere the especial bearer of therapeutic influences, and it works the stronger the less one knows

¹S. Ferenczi: Introduction und Uebertragung. Jahrbuch f. psychoanal. u. psychopath. Forschungen, Bd. I, H. 2., 1909.

of its presence. Accordingly psychoanalysis does not create it, it merely discloses it to consciousness, and avails itself of it, in order to direct the psychic processes to the wished for goal. But I cannot leave the theme of transfer without stressing the fact that this phenomenon is of decisive importance to convince not only the patient, but also the physician. I know that all my adherents were first convinced of the correctness of my views through their experience with transfer, and I can very well conceive that one may not win such a surety of judgment so long as he makes no psychoanalysis, and so has not himself observed the effects of transfer.

Ladies and gentlemen, I am of the opinion that there are, on the intellectual side, two hindrances to acknowledging the value of the psychoanalytic view-point: first, the fact that we are not accustomed to reckon with a strict determination of mental life, which holds without exception, and second, the lack of knowledge of the peculiarities through which unconscious mental processes differ from those conscious ones with which we are familiar. One of the most widespread resistances against the work of psychoanalysis with patients as with persons in health reduces to the latter of the two moments. One is afraid of doing harm by psychoanalysis, one is anxious about calling up into consciousness the repressed sexual impulses of the patient, as though there were danger that they could overpower the higher ethical strivings and rob him of his cultural acquisitions. One can see that the patient has sore places in his soul life, but one is afraid to touch them, lest his suffering be increased. We may use this analogy. It is, of course, better not to touch diseased places when one can only cause pain. But we know that the surgeon does not refrain from the investigation and reinvestigation of the seat of illness, if his invasion has as its aim the restoration of lasting health. Nobody thinks of blaming him for the unavoidable difficulties of the investigation or the phenomena of reaction from the operation, if these only accomplish their purpose, and gain for the patient a final cure by temporarily making his condition worse. The case is similar in psychoanalysis; it can lay claim to the same things as surgery; the increase of pain which takes place in the patient during the treatment is very much less than that which the surgeon imposes upon him, and especially negligible in comparison with the pains of serious illness. But the consequence which is feared, that of a disturbance of the cultural character by the impulse which has been freed from repression, is wholly impossible. In relation to this anxiety we must consider what our experiences have taught us with certainty, that the somatic and mental power of a wish, if once its repression has not succeeded, is incom-

parably stronger when it is unconscious than when it is conscious, so that by being made conscious it can only be weakened. The unconscious wish cannot be influenced, is free from all strivings in the contrary direction, while the conscious is inhibited by those wishes which are also conscious and which strive against it. The work of psychoanalysis accordingly presents a better substitute, in the service of the highest and most valuable cultural strivings, for the repression which has failed.

Now what is the fate of the wishes which have become free by psychoanalysis, by what means shall they be made harmless for the life of the individual? There are several ways. The general consequence is, that the wish is consumed during the work by the correct mental activity of those better tendencies which are opposed to it. The repression is supplanted by a condemnation carried through with the best means at one's disposal. This is possible, since for the most part we have to abolish only the effects of earlier developmental stages of the ego. The individual for his part only repressed the useless impulse, because at that time he was himself still incompletely organized and weak; in his present maturity and strength he can, perhaps, conquer without injury to himself that which is inimical to him. A second issue of the work of psychoanalysis may be that the revealed unconscious impulses can now arrive at those useful applications which, in the case of undisturbed development, they would have found earlier. The extirpation of the infantile wishes is not at all the ideal aim of development. The neurotic has lost, by his repressions, many sources of mental energy whose contingents would have been very valuable for his character building and his life activities. We know a far more purposive process of development, the so-called *sublimation* (*Sublimierung*), by which the energy of infantile wish-excitations is not secluded, but remains capable of application, while for the particular excitations, instead of becoming useless, a higher, eventually no longer sexual, goal is set up. The components of the sexual instinct are especially distinguished by such a capacity for the sublimation and exchange of their sexual goal for one more remote and socially more valuable. To the contributions of the energy won in such a way for the functions of our mental life we probably owe the highest cultural consequences. A repression taking place at an early period excludes the sublimation of the repressed impulse; after the removal of the repression the way to sublimation is again free.

We must not neglect, also, to glance at the third of the possible issues. A certain part of the suppressed libidinous excitation has a right to direct satisfaction and ought to find

it in life. The claims of our civilization make life too hard for the greater part of humanity, and so further the aversion to reality and the origin of neuroses, without producing an excess of cultural gain by this excess of sexual repression. We ought not to go so far as to fully neglect the original animal part of our nature, we ought not to forget that the happiness of individuals cannot be dispensed with as one of the aims of our culture. The plasticity of the sexual-components, manifest in their capacity for sublimation, may cause a great temptation to accomplish greater culture-effects by a more and more far reaching sublimation. But just as little as with our machines we expect to change more than a certain fraction of the applied heat into useful mechanical work, just as little ought we to strive to separate the sexual impulse in its whole extent of energy from its peculiar goal. This cannot succeed, and if the narrowing of sexuality is pushed too far it will have all the evil effects of a robbery.

I do not know whether you will regard the exhortation with which I close as a presumptuous one. I only venture the indirect presentation of my conviction, if I relate an old tale, whose application you may make yourselves. German literature knows a town called Schilda, to whose inhabitants were attributed all sorts of clever pranks. The wiseacres, so the story goes, had a horse, with whose powers of work they were well satisfied, and against whom they had only one grudge, that he consumed so much expensive oats. They concluded that by good management they would break him of this bad habit, by cutting down his rations by several stalks each day, until he had learned to do without them altogether. Things went finely for a while, the horse was weaned to one stalk a day, and on the next day he would at last work without fodder. On the morning of this day the malicious horse was found dead; the citizens of Schilda could not understand why he had died. We should be inclined to believe that the horse had starved, and that without a certain ration of oats no work could be expected from an animal.

I thank you for calling me here to speak, and for the attention which you have given me.

THE ASSOCIATION METHOD¹

By PROFESSOR CARL G. JUNG

Ladies and Gentlemen: When I was honored with the invitation from Clark University to lecture before this esteemed assemblage, a wish was at the same time expressed that I should speak about my methods of work, and especially about the psychology of childhood. I hope to accomplish this task in the following manner:

In my first lecture I shall try to present to you the view points of my association methods; in my second lecture I shall discuss the significance of the familiar constellations; while in my third lecture I shall enter more fully into the psychology of the child.

I might easily confine myself exclusively to my theoretical views, but I believe that it will be better to illustrate my lectures with as many practical examples as possible. We shall therefore occupy ourselves first with the method of association, a method which has been of valuable assistance to me both practically and theoretically. The association method in vogue in psychology, as well as its history, is of course, so familiar to you that there is no need to speak of it. For practical purposes I make use of the following formulary:

- | | | |
|--------------|--------------|---------------|
| 1. head | 19. pride | 37. salt |
| 2. green | 20. to cook | 38. new |
| 3. water | 21. ink | 39. custom |
| 4. to sing | 22. angry | 40. to pray |
| 5. dead | 23. needle | 41. money |
| 6. long | 24. to swim | 42. foolish |
| 7. ship | 25. voyage | 43. pamphlet |
| 8. to pay | 26. blue | 44. despise |
| 9. window | 27. lamp | 45. finger |
| 10. friendly | 28. to sin | 46. expensive |
| 11. to cook | 29. bread | 47. bird |
| 12. to ask | 30. rich | 48. to fall |
| 13. cold | 31. tree | 49. book |
| 14. stem | 32. to prick | 50. unjust |
| 15. to dance | 33. pity | 51. frog |
| 16. village | 34. yellow | 52. to part |
| 17. lake | 35. mountain | 53. hunger |
| 18. sick | 36. to die | 54. white |

¹ Lectures delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909; translated from the German by Dr. A. A. Brill, of New York.

55. child	71. flower	86. false
56. to take care	72. to beat	87. anxiety
57. lead pencil	73. box	88. to kiss
58. sad	74. wild	89. bride
59. plum	75. family	90. pure
60. to marry	76. to wash	91. door
61. house	77. cow	92. to choose
62. dear	78. friend	93. hay
63. glass	79. luck	94. contented
64. to quarrel	80. lie	95. ridicule
65. fur	81. deportment	96. to sleep
66. big	82. narrow	97. month
67. carrot	83. brother	98. nice
68. to paint	84. to fear	99. woman
69. part	85. stork	100. to abuse
70. old		

This formulary has been constructed after many years of experience. The words are chosen and partially arranged in such a manner as to strike easily almost all complexes of practical occurrence. As shown by the above formulary there is a regular mixing of the grammatical qualities of the words. This, too, has its definite reasons.¹

Before the experiment begins the test person receives the following instruction: "Answer as quickly as possible the first word that occurs to your mind." This instruction is so simple that it can easily be followed by anybody. The work itself, moreover, appears extremely easy, so that it might be expected that any one could accomplish it with the greatest facility and promptitude. But contrary to expectation the behavior is quite different.

An Example of a Normal Reaction Type

Stimulus word	Reaction Time Unit 0.2 second	Reaction	Reproduction
head	9	foot	part of the body
green	11	blouse	
water	14	clear	light
to sing	6	children	
dead	11	do not like	
long	6	short	I, tall
ship	7	forth	
to pay	9	bills	
window	9	room	
friendly	10	children	
table	9	chair	room
to ask	10	all kinds	
cold	7	warm	

¹The selection of these stimulus words was naturally made for the German language only, and would probably have to be considerably changed for the English language.

An Example of Normal Reaction Type—Cont.

Stimulus word	Reaction Time Unit 0.2 second	Reaction	Reproduction
stem	6	flower	like
to dance	9	I	
lake	8	Zurich	
sick	8	sister	
pride	6	people	people
to cook	7	woman	
ink	5	black	
angry	10	children	
needle	9	to prick	like
to swim	10	healthy	
voyage	9	England	
blue	10	pretty	
lamp	6	light	people like, necessary
to sin	8	much	
bread	10	good	
rich	9	nice	
tree	6	green	
to prick	9	need	

An Example of an Hysterical Reaction Type

Stimulus word	Reaction Time Unit 0.2 second	Reaction	Reproduction
needle	7	to sew	ship
to swim	9	water	
* †			
voyage	35	to ride, motion, voyager	
blue	10	color	possession green
lamp	7	to burn	
to sin	22	this idea is totally strange to me, I do not recognize it	
bread	10	to eat	
rich †	50	money, I don't know	NaCl as an opposite barbaric
brown	6	nature	
to prick	9	needle	
pity	12	feeling	
yellow	9	color	?
mountain	8	high	
to die	8	to perish	
salt	15	salty (laughs) I don't know	
new	15	old	
custom	10	good	
to pray	12	Deity	
money	10	wealth	
foolish	12	narrow minded, restricted	
pamphlet	10	paper	

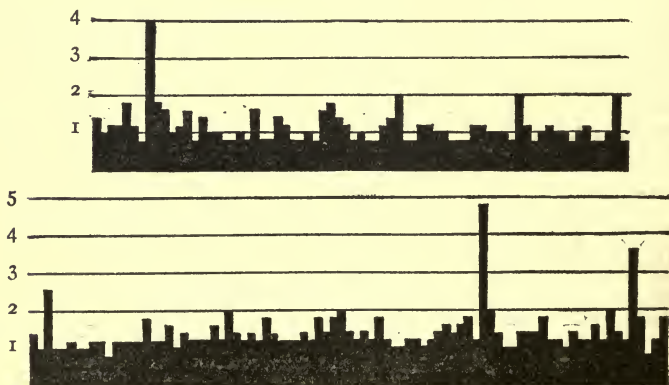
* Denotes misunderstanding.

† Denotes repetition of the stimulus words.

An Example of an Hysterical Reaction Type—Cont.

Stimulus word	Reaction Time Unit 0.2 second	Reaction	Reproduction
despise	30	that is a complicated, too foolish	?
finger	8	hand, not only hand, but also foot, a joint, member, extremity	
dear	14	to pay (laughs)	?
bird	8	to fly	
to fall	30	tomber, I will say no more, what do you mean by fall?	?
book	6	to read	
unjust	8	just	?
frog	11	quack	
to part	30	what does part mean?	?
hunger	10	to eat	
white	12	color, everything possible, light	?
child	10	little, I did not hear well, bébé	
to take care	14	attention	to be
lead pencil	8	to draw, everything possible can be drawn	
sad	9	to weep, that is not always the case	fruit
plum	16	to eat a plum, pluck what do you mean by it? Is that symbolic?	
to marry	27	how can you? reunion, union	union alliance

The following curves illustrate the course of the reaction time in an association experiment in four normal test persons. The length of each column denotes the length of the reaction time.

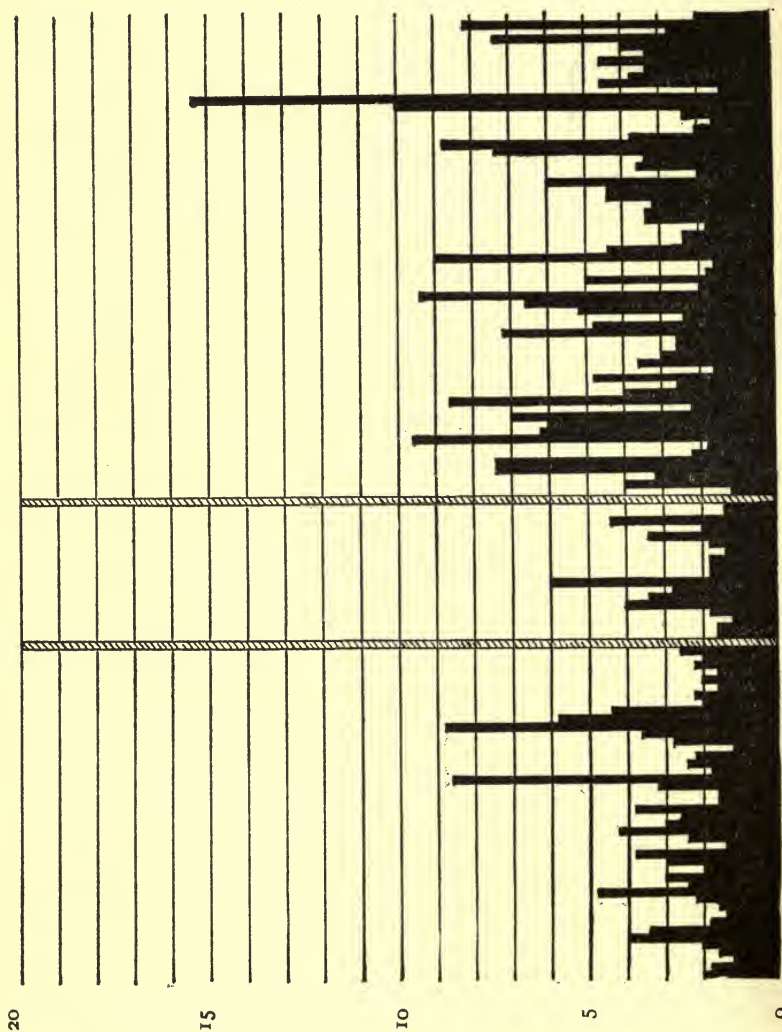




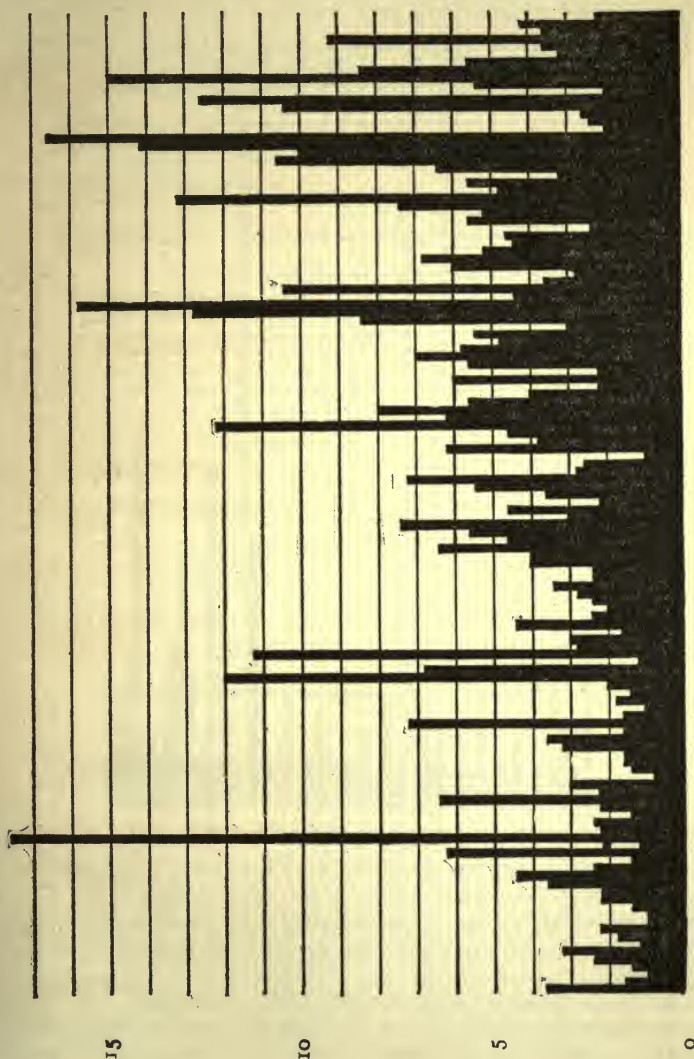
The illustrations below (pp. 224 ff.) show the course of the reaction time in hysterical individuals. The light cross-hatched columns denote the locations where the test person was unable to react (so-called failures).

The first thing that strikes us is the fact that many test persons show a marked prolongation of the reaction time. This would make us think at first of intellectual difficulties,—wrongly, however, as we are often dealing with very intelligent persons of fluent speech. The explanation lies rather in the emotions. In order to understand the matter comprehensively we must bear in mind that the association experiments cannot deal with a separated psychic function, for any psychic occurrence is never a thing in itself, but is always the resultant of the entire psychological past. The association experiment, too, is not merely a method for the reproduction of separated word couplets, but it is a kind of pastime, a conversation between experimenter and test person. In a certain sense it is even still more than that. Words are really something like condensed actions, situations, and things. When I present a word to the test person which denotes an action it is the same as if I should present to him the action itself, and ask him, “How do you behave towards it? What do you think of it? What do you do in this situation?” If I were a magician I should cause the situation corresponding to the stimulus word to appear in reality and placing the test person in its midst, I should then study his manner of reaction. The result of my stimulus words would thus undoubtedly approach infinitely nearer perfection. But as we are not magicians we must be contented with the linguistic substitutes for reality; at the same time we

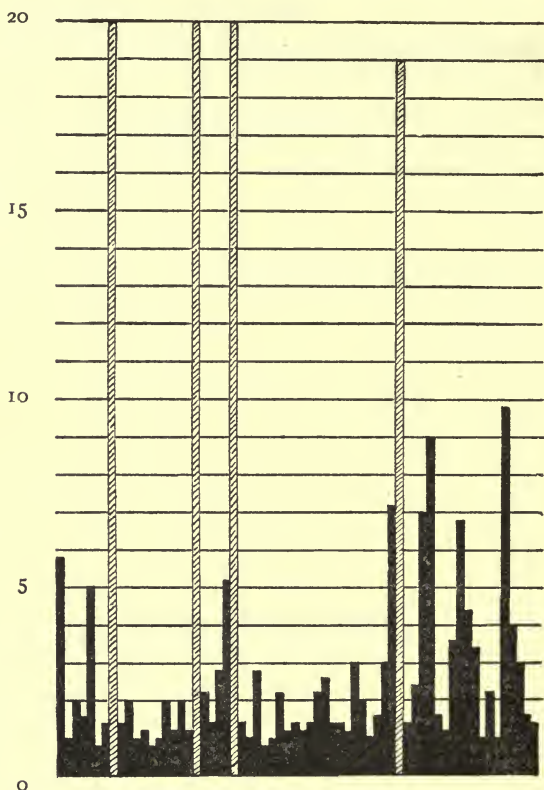
✓ must not forget that the stimulus word will as a rule always conjure up its corresponding situation. It all depends on how the test person reacts to this situation. The situation "bride" or "bridegroom" will not evoke a simple reaction in a young lady; but the reaction will be deeply influenced by the provoked strong feeling tones, the more so if the experimenter be a man. It thus happens that the test person is often unable to react



quickly and smoothly to all stimulus words. In reality, too, there are certain stimulus words which denote actions, situations, or things, about which the test person cannot think quickly and surely, and this fact is shown in the association experiments. The example which I have just presented shows an abundance of long reaction times and other disturbances.



✓ In this case the reaction to the stimulus word is in some way impeded, that is, the adaptation to the stimulus word is disturbed. The stimulus words are therefore merely a part of reality acting upon us; indeed, a person who shows such disturbances to the stimulus words, is in a certain sense really but imperfectly adapted to reality. Disease is an imperfect adaptation; hence in this case we are dealing with something morbid



✓ in the psyche,—with something which is either temporary or persistently pathological, that is, we are dealing with a psychoneurosis, with a functional disturbance of the mind. This rule, however, as we shall see later, is not without its exceptions.

Let us in the first place continue the discussion concerning the prolonged reaction time. It often happens that the test person actually does *not* know what to answer to the stimulus

word. The test person waives any reaction; for the moment he totally fails to obey the original instructions, and shows himself incapable of adapting himself to the experimenter. If this phenomenon occurs frequently in an experiment it signifies a higher degree of disturbance in adjustment. I call attention to the fact that it is quite indifferent what reason the test person gives for the refusal. Some find that too many ideas suddenly occur to them, others, that not enough ideas come to their minds. In most cases, however, the difficulties first perceived are so deterrent that they actually give up the whole reaction. The following example shows a case of hysteria with many failures of reaction:

Stimulus word	Reaction Time Unit 0.2 second	Reaction	Reproduction
to sing	9	nice	+
dead	15	awful	?
long*	40	the time, the journey	?
ship †			+
to pay	11	money	
window	10	big	high
friendly	50	a man	human
to cook	10	soup	+
ink	9	black or blue	+
angry			bad
needle	9	to sew	+
lamp	14	light	+
to sin			
bread	15	to eat	+
rich* †	40	good, convenient	+
yellow	18	paper	color
mountain	10	high	+
to die	15	awful	+
salt †	25	salty	+
new			good, nice
custom †			
to pray			
money †	35	to buy, one is able	+
pamphlet	16	to write	+
to despise †	22	people	+
finger †			
dear	12	thing	+
bird	12	sings or flies	+

* Donates misunderstanding.

† Denotes repetition of the stimulus words.

In example 3 we find a characteristic phenomenon. The test person is not content with the requirements of the instruction, that is, she is not satisfied with *one* word but reacts with many words. She apparently does more and better than the instruction requires, but in so doing she does not fulfill the requirements of the instruction. Thus she reacts:—custom—good—barbaric; foolish—narrow minded—restricted; family—big—small—everything possible.

These examples show in the first place that many other words connect themselves with the reaction word. The test person is unable to suppress the ideas which subsequently occur to her. In doing this she also pursues a certain tendency which perhaps is more distinctly expressed in the following reaction: new—old—as an opposite. The addition of “as an opposite” denotes that the test person has the desire to add something explanatory or supplementary. This tendency is also shown in the following reaction: finger—not only hand, also foot—a limb—member—extremity.

Here we have a whole series of supplements. It seems as if the reaction were not sufficient for the test person, as if something else must always be added, as if what has been already said were incorrect or in some way imperfect. This feeling we may with Janet designate as the *‘sentiment d’incomplétude,’* which by no means explains everything. I enter somewhat deeper into this phenomenon because it is quite frequently encountered in neurotic individuals. Indeed it is not merely a small and unimportant subsidiary manifestation in an insignificant experiment, but rather an elemental and universal manifestation which otherwise plays a rôle in the psychic life of neurotics.

With his desire to supplement the test person betrays a tendency to give the experimenter more than he wants, he even exerts the greatest efforts to seek further mental occurrences in order finally to discover something quite satisfactory. If we translate this elementary observation into the psychology of everyday life, it signifies that the test person has a tendency constantly to give to others more feeling than is required and expected. According to Freud, this is a sign of a reinforced object-libido, that is, it is a compensation for an inner unsatisfaction and voidness of feeling. In this elementary observation we therefore see one of the main qualities of hysterics, namely, the tendency to allow themselves to be carried away by everything, to attach themselves enthusiastically to everything, and to always promise too much and hence do little. Patients having this symptom, in my experience, are always hard to deal with; at first they are enthusiastically enraptured with the physician, for a time going so far as to accept everything blindly; but they soon merge into just as blind a resistance against the physician, thus rendering any educative influence absolutely impossible.

We see therefore in this phenomenon the expression of a tendency to give more than the instruction demands and expects. This tendency betrays itself also in other failures to follow the instruction:

to quarrel—angry—different things—I always quarrel at home;

to marry—how can you marry?—reunion—union;

plum—to eat—to pluck—what do you mean by it?—is it symbolic?

to sin—this idea is quite strange to me, I do not recognize it.

These reactions show that the test person gets away altogether from the situation of the experiment. For the instruction demands that he should answer only the word which next occurs to him. Here we find that the stimulus words apparently act with excessive strength, that they are taken as if they were direct personal questions. The test person entirely forgets that we deal with mere words which stand in print before us, and seeks in them a personal meaning; he tries to divine them and defend himself against them, thus altogether forgetting the instructions.

This elementary observation depicts another common peculiarity of hysterics, namely, that of taking everything personally, of never being able to remain objective, and of allowing themselves to be carried away by momentary impressions; this again shows the characteristics of the enhanced object-libido.

Another sign of impeded adaptation is the often occurring *repetitions of the stimulus words*. The test persons repeat the stimulus word as if they had not heard or understood it distinctly. They repeat it just as we repeat a difficult question in order better to grasp it before answering. This same tendency is shown in the experiment. The questions are repeated because the stimulus words act on hysterical individuals almost like difficult and personal questions. In principle it is the same phenomenon as the subsequent completion of the reaction.

In many experiments we observe that the same reaction constantly reappears to the most varied stimulus words. These words seem to possess a special reproduction tendency, and it is very interesting to examine their relationship to the test person. For example, I have observed a case in which the patient repeated the word "short" a great many times and often in places where it had no meaning. The test person could not directly state the reason for the repetition of the word "short." From experience I knew that such predicates always relate either to the test person himself or to the person nearest to him. I assumed that in this word "short" he designated himself, and that in this way he helped to express something very painful to him. The test person is of very small stature. He is the youngest of four brothers, who in contrast to him are all tall. He was always the "*child*" in the family, he was nicknamed "Short" and was treated by all as the "little one." This resulted in a total loss of self-confidence. Although he was intelligent, and despite long

study, he could not decide to present himself for examination; he finally became impotent, and merged into a psychosis in which, whenever he was alone, he took delight in walking about in his room on his toes in order to appear taller. The word "short," therefore, signified to him a great many painful experiences. This is usually the case with the repeated words; they always contain something very important for the individual psychology of the test person.

The signs thus far depicted are not found arbitrarily spread throughout the whole experiment, but only in very definite locations; namely, in those stimulus words which strike against special emotionally accentuated complexes. This fact is the foundation of the so-called "diagnosis of facts" (*Tatbestandsdiagnostik*); that is, of the method employed to discover by means of an association experiment, the culprit among a number of persons suspected of a crime. That this is possible I should like to demonstrate briefly in a concrete case.

On the 6th of February, 1908, our supervisor reported to me that a nurse complained to her of having been robbed during the forenoon of the previous day. The facts were as follows: The nurse kept her money, amounting to 70 francs, in a pocketbook which she had placed in her cupboard where she also kept her clothes. The cupboard contained two compartments, of which one belonged to the nurse who was robbed, and the other to the head nurse. These two nurses and a third one, who was an intimate friend of the head nurse, slept in the same room where the cupboard was. The room was in a section which was regularly occupied in common by six nurses who had free access to this room. Given such a state of affairs it is not to be wondered that the supervisor shrugged her shoulders when I asked her whom she most suspected.

Further investigation showed that on the morning of the theft the above-mentioned friend of the head nurse was slightly indisposed and remained in bed in the room the whole morning. Hence, following the indications of the plaintiff, the theft could have taken place only in the afternoon. Of the other four nurses upon whom suspicion could fall, there was only one who regularly attended to the cleaning of the room in question, while the remaining three had nothing to do in this room, nor was it shown that any of them had spent any time there on the previous day.

It was therefore natural that these last three nurses should be regarded for the time being as less implicated, and I therefore began by subjecting the first three to the experiment.

From the particulars of the case, I also knew that the cupboard was locked but that the key was kept not far away in a

very conspicuous place, that on opening the cupboard the first thing to be seen was a fur ornament (boa), and, moreover, that the pocketbook was between the linen in an inconspicuous place. The pocketbook was of dark reddish leather, and contained the following objects: one 50 franc banknote, one 20 franc piece, some centimes, one small silver watch chain, one stencil used in the insane asylum to mark the kitchen utensils, and one small receipt from Dosenbach's shoeshop in Zürich.

Besides the plaintiff and the guilty one, only the head nurse knew the exact particulars of the deed, for as soon as the former missed her money she immediately asked the head nurse to help her find it, thus the head nurse had been able to learn the smallest details, which naturally rendered the experiment still more difficult, for she was precisely the one most suspected. The conditions for the experiment were better for the others, since they knew nothing concerning the particulars of the deed, and some not even that a crime had been committed. As critical stimulus words I selected the name of the robbed nurse, plus the following words: cupboard, door, open, key, yesterday, banknote, gold, 70, 50, 20, money, watch, pocketbook, chain, silver, to hide, fur, dark reddish, leather, centimes, stencil, receipt, Dosenbach. Besides these words which referred directly to the deed, I took also the following, which had a special affective value: theft, to take, to steal, suspicion, blame, court, police, to lie, to fear, to discover, to arrest, innocent.

The objection is often made to the last species of words that they may produce a strong affective resentment even in innocent persons, and for that reason one cannot attribute to them any comparative value. Nevertheless, it may always be questioned whether the affective resentment of an innocent person will have the same effect on the association as that of a guilty one, and that question can only be authoritatively answered by experience. Until the contrary shall be demonstrated, I maintain that even words of the above mentioned type may profitably be used.

I then distributed these critical words among twice as many indifferent stimulus words in such a manner that each critical word was followed by two indifferent ones. As a rule it is well to follow up the critical words by indifferent words in order that the action of the first may be clearly distinguished. But one may also follow up one critical word by another, especially if one wishes to bring into relief the action of the second. Thus I placed together "darkish red" and "leather," and "chain" and "silver."

After this preparatory work I undertook the experiment

with the three above mentioned nurses. As examinations of this kind can be rendered into a foreign tongue only with the greatest difficulty, I will content myself with presenting the general results, and with giving some examples. I first undertook the experiment with the friend of the head nurse, and judging by the circumstances she appeared only slightly moved. The head nurse was next examined; she showed marked excitement, her pulse being 120 per minute immediately after the experiment. The last to be examined was the nurse who attended to the cleaning of the room in which the theft occurred. She was the most tranquil of the three; she displayed but little embarrassment, and only in the course of the experiment did it occur to her that she was suspected of stealing, a fact which manifestly disturbed her towards the end of the experiment.

The general impression from the examination spoke strongly against the head nurse. It seemed to me that she evinced a very "suspicious," or I might almost say, "impudent" countenance. With the definite idea of finding in her the guilty one I set about adding up the results.

One can make use of many special methods of computing, but they are not all equally good and equally exact. (One must always resort to calculation, as appearances are enormously deceptive.) The method which is most to be recommended is that of the probable average of the reaction time. It shows at a glance the difficulties which the person in the experiment had to overcome in the reaction.

The technique of this calculation is very simple. The probable average is the middle number of the various reaction times arranged in a series. The reaction times are, for example,¹ placed in the following manner: 5, 5, 5, 7, 7, 7, 8, 9, 9, 9, 12, 13, 14. The number found in the middle (8) is the probable average of this series. Following the order of the experiment, I shall denote the friend of the head nurse by the letter A, the head nurse by B, and the third nurse by C.

The probable averages of the reaction are:

A	B	C
10.0	12.0	13.5

No conclusions can be drawn from this result. But the average reaction times calculated separately for the indifferent reactions, for the critical, and for those immediately following the critical (post-critical) are more interesting.

From this example we see that whereas A has the shortest reaction time for the indifferent reactions, she shows in com-

¹ Reaction times are always given in fifths of a second.

The Probable Average of the Reaction Time

for	A	B	C
Indifferent reactions . . .	10.0	11.0	12.0
Critical reactions . . .	16.0	13.0	15.0
Post-critical reactions . . .	10.0	11.0	13.0

parison to the other two persons of the experiment, the longest time for the critical reactions.

The difference between the reaction times, let us say between the indifferent and the critical, is 6 for A, 2 for B, and 3 for C, that is, it is more than double for A when compared with the other two persons.

In the same way we can calculate how many complex indicators there are on an average for the indifferent, critical, etc., reactions.

The Average Complex Indicators for each Reaction

for	A	B	C
Indifferent reactions . . .	0.6	0.9	0.8
Critical reactions . . .	1.3	0.9	1.2
Post critical reactions . . .	0.6	1.0	0.8

The difference between the indifferent and critical reactions for $A=0.7$, for $B=0$, for $C=0.4$. A is again the highest.

Another question to consider is, in what special way do the imperfect reactions behave?

The result for $A=34\%$, for $B=28\%$, and for $C=30\%$.

Here, too, A reaches the highest value, and in this, I believe, we see the characteristic moment of the guilt-complex in A. I am, however, unable to explain here circumstantially the reasons why I maintain that memory errors are related to an emotional complex, as this would lead me beyond the limits of the present work. I therefore refer the reader to my work "*Ueber die Reproduktionsstörungen im Associationsexperiment*" (IX Beitrag der Diagnost. Associat. Studien).

As it often happens that an association of strong feeling tone produces in the experiment a perseveration, with the result that not only the critical association, but also two or three successive

associations are imperfectly reproduced, it will be very interesting for our cases to see how many imperfect reproductions are so arranged in the series. The result of computation shows that the imperfect reproductions thus arranged in series are for A 64.7%, for B 55.5%, and for C 30.0%.

Again we find that A has the greatest percentage. To be sure this may partially depend on the fact that A also possesses the greatest number of imperfect reproductions. Given a small quantity of reactions it is usual that the greater the total number of the same the more imperfect reactions will occur in groups. But in order that this should be probable it could not occur in so great a measure as in our case, where on the other hand B and C have not a much smaller number of imperfect reactions when compared to A. It is significant that C with her slight emotions during the experiment shows the minimum of imperfect reproductions arranged in series.

As imperfect reproductions are also complex indicators, it is necessary to see how they distribute themselves in respect to the indifferent, critical, etc., reactions.

Imperfect Reproductions which occur

in	A	B	C
Indifferent reactions . . .	10	12	11
Critical reactions	19	9	12
Post-critical reactions . . .	5	7	7

It is hardly necessary to bring into prominence the differences between the indifferent and the critical reactions of the various subjects as shown by the resulting numbers of the table. In this respect, too, A occupies first place.

Naturally, here, too, there is a probability that the greater the quantity of the imperfect reproductions the greater is their number in the critical reactions. If we suppose that the imperfect reproductions are distributed regularly and without choice among all the reactions there will be a greater number of them for A (in comparison to B and C) even as reactions to critical words, since A has the greater number of imperfect reproductions. Admitting such a uniform distribution of the imperfect reproductions, it is easy to calculate how many we ought to expect to belong to each individual kind of reaction.

From this calculation it appears that the disturbances of reproductions which concern the critical reactions for A surpass by far the expected, for C they are 0.9 higher than the ex-

Imperfect Reproductions

Which may be expected				Which really occur		
For	Indifferent Reactions	Critical Reactions	Post-critical Reactions	Indifferent Reactions	Critical Reactions	Post-critical Reactions
A	11.2	12.5	10.2	10	19	5
B	9.2	10.3	8.4	12	9	7
C	9.9	11.1	9.0	11	12	7

pected, while for B the real number is less than the one expected.

All this points to the fact that in the subject A the critical stimulus words acted with the greatest intensity, and hence the greatest suspicion falls on A. Practically one may venture to designate such a subject as probably guilty. The same evening A made a complete confession of the theft, and thus the success of the experiment was confirmed.

I maintain that such a result should be of scientific interest and worthy of consideration. There is much in experimental psychology which is less useful than the material treated in this work. Putting aside altogether the theoretical interest, we have in this case something that is not to be despised from a practical point of view, to wit, we have brought to light the culpable affair in a much easier and shorter way than is customary. What has been possible once or twice ought to be possible again in other cases, and it is well worth while to investigate the means of rendering the method increasingly capable of rapid and sure results.

This applicability of the experiment shows it possible to strike a concealed (indeed an unconscious) complex by means of a stimulus word; and conversely we may assume with great certainty that behind a reaction which shows a complex indicator there is a hidden complex, even though the test person strongly denies it. One must get rid of the idea that educated and intelligent test persons are able to see and admit their own complexes. Every human mind contains much that is unacknowledged and hence unconscious as such; and no one can boast that he stands completely above his complexes. Those who persist in maintaining it do not see the spectacles which they wear on their noses.

It has long been thought that the association experiment

enables one to distinguish certain *intellectual* types. That is not the case. The experiment does not give us any particular insight into the purely intellectual, but rather only into the emotional processes. To be sure we can erect certain types of reaction; they are not, however, based on intellectual peculiarities, but depend entirely on the *proportionate emotional state*. Educated test persons usually show superficial and linguistically deep rooted associations, whereas the uneducated form more valuable associations and often of ingenious significance. This behavior would be paradoxical from an intellectual viewpoint. The meaningful associations of the uneducated are not really the product of intellectual thinking, but are simply the results of a special emotional state. The whole thing is more important to the uneducated, his emotion is greater and for that reason he pays more attention to the experiment than the educated person, and that is why his associations are more significant. Aside from the types determined by education we have to consider three principal individual types:

1. An objective type with undisturbed reactions.
2. A so-called complex type with many disturbances in the experiment occasioned by the constellation of a complex.
3. A so-called definition-type. This type consists in the fact that the reaction always gives an explanation or a definition of the content of the stimulus word; *e. g.*:

apple,—a tree-fruit;
table,—a piece of household furniture;
to promenade,—an activity;
father,—chief of the family.

This type is chiefly found in stupid persons, and it is therefore quite usual in imbecility. But it can also be found in persons who are not really stupid, but who do not wish *to be taken as stupid*. Thus a young student from whom associations were taken by an older intelligent woman student reacted altogether with definitions. The test person was of the opinion that it was an examination in intelligence, and therefore directed most of his attention to the significance of the stimulus words; his associations, therefore, looked like those of an idiot. Not all idiots, however, react with definitions; probably only those so react who would like to appear smarter than they are, that is, those to whom their stupidity is painful. I designate this widespread complex as "intelligence-complex." A normal test person reacts in a most overdrawn manner as follows:

anxiety—heart anguish;
to kiss—love's unfolding;
to kiss—perception of friendship.

This type gives a constrained and unnatural impression. The test persons wish to be more than they are, they wish to

exert more influence than they really have. Hence we see that persons with an intelligence complex are usually not natural and unconstrained; that they are always somewhat unnatural and flowery; they show a predilection for complicated foreign words, high sounding quotations, and other intellectual ornaments. In this way they wish to influence their fellow beings, they wish to impress others with their apparent education and intelligence, and thus to compensate for the painful feeling of stupidity. The definition type is closely related to the predicate type, or to express it more precisely, to the predicate type expressing personal judgment (*Wertprädikativtypus*). For example: flower—pretty;

money—convenient;
animal—ugly;
knife—dangerous;
death—ghastly.

In the definition type the intellectual significance of the stimulus word is rendered prominent, while in the predicate type it is its *emotional significance*. There are predicate types which are altogether overdrawn where there appear reactions like the following:

piano—horrible;
to sing—heavenly;
mother—ardently loved;
father—something good, nice, holy.

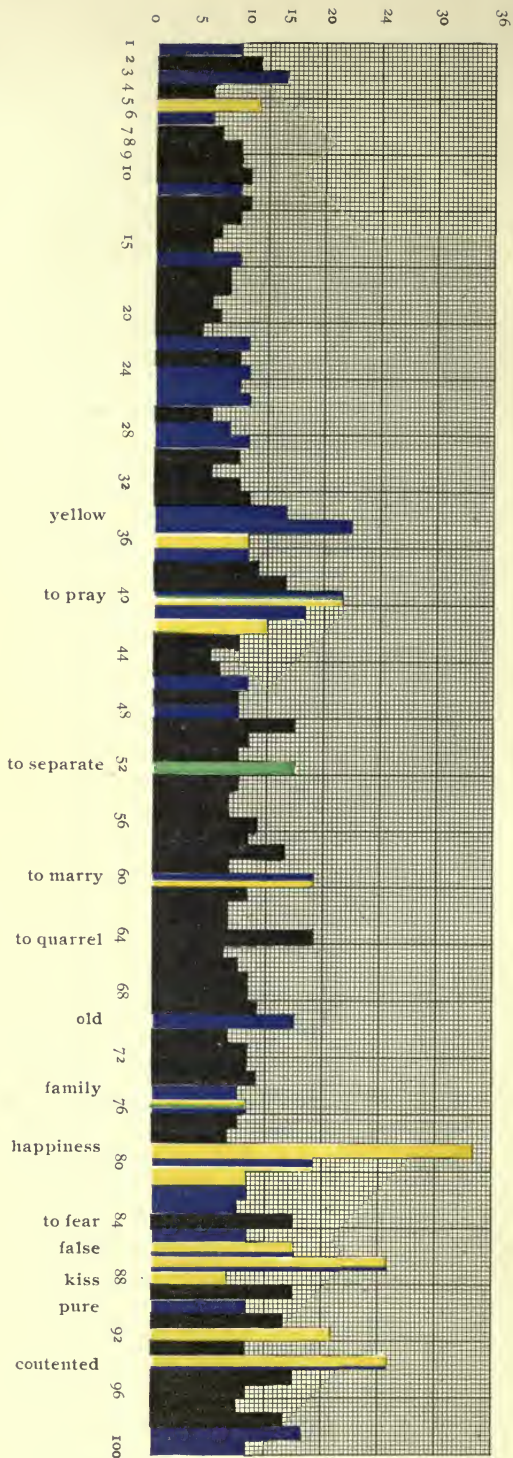
In the definition type an absolute *intellectual* make-up is manifested or rather simulated, but here there is a very *emotional* one. Yet, just as the definition type really conceals a lack of intelligence so the excessive *emotional* expression conceals or overcompensates an emotional deficiency. This conclusion is very interestingly illustrated by the following discovery:—On investigating the influence of the familiar milieus on the association type it was found that young individuals seldom possess a predicate type, but that on the other hand, the predicate type increases in frequency with the advancing age. In women the increase of the predicate type begins a little after the 40th year, and in men after the 60th. That is the precise time when, owing to the deficiency of sexuality, there actually occurs considerable emotional loss. If a test person evinces a distinct predicate type it may always be inferred that a marked internal emotional deficiency is thereby compensated. Still one cannot reason conversely, namely that an inner emotional deficiency must produce a predicate type, no more than that idiocy directly produces a definition type. A predicate type can also betray itself through the external behavior, as, for example, through a particular affectation, enthusiastic exclamations, an embellished behavior, and the constrained sounding language so often observed in society.

The complex type shows no particular tendency except the *concealment* of a complex, whereas the definition and predicate types betray a positive tendency to exert in some way a *definite* influence on the experimenter. But whereas the definition type tends to bring to light its intelligence, the predicate type displays its emotion. I need hardly add of what importance such determinations are for the diagnosis of character.

After finishing an association experiment I usually add another experiment of a different kind, the so-called *reproduction*. I repeat the same stimulus words and ask the test persons whether they still remember their former reactions. In many instances the memory fails, and as experience shows, these locations are stimulus words which touched an emotionally accentuated complex, or stimulus words immediately following such critical words.

This phenomenon has been designated as paradoxical and contrary to all experience. For it is known that emotionally accentuated things are better retained in memory than indifferent things. This is quite true, but it does not hold for the *linguistic* expression of an emotionally accentuated content. On the contrary one very easily forgets what he has said under emotion, one is even apt to contradict himself about it. Indeed the efficacy of cross-examinations in court depends on this fact. The reproduction method therefore serves to render still more prominent the complex stimulus. In normal persons we usually find a limited number of false reproductions, seldom more than 10-20%, while in abnormal persons, especially in hysterics, we often find from 20-40% of false reproductions. The reproduction certainty is therefore in certain cases a measure for the emotivity of the test person.

By far the larger number of neurotics show a pronounced tendency to cover up their intimate affairs in impenetrable darkness, even from the doctor, so that the doctor finds it very difficult to form a proper picture of the patient's psychology. In such cases I am greatly assisted by the association experiment. When the experiment is finished I first look over the general course of the reaction times. I see a great many very prolonged times which in itself means that the patient can only adjust himself with difficulty, that his psychological functions proceed with marked internal frictions, with *resistances*. By far the greater number of neurotics react only under great and hence very distinct resistances, there are, however, cases in which the average reaction times are as short as in the normal and in whom the other complex indicators are lacking, but who, despite that fact, undoubtedly present neurotic symptoms. These rare cases are especially found among very intelligent



For the stimulus words corresponding to the numbers see the formulary on the first and second pages of Lecture I.

and educated chronic patients who after many years of practice have learned to control their outward behavior and therefore outwardly display very little if anything of their neuroses. On superficial observation they can be taken as normal, yet in some places they show disturbances which betray the repressed complex.

After examining the reaction times I turn my attention to the type of the association to ascertain with what type I am dealing. If it is a predicate type I draw the conclusions which I have detailed above; if it is a complex type I try to ascertain the nature of the complex. With the necessary experience one can readily emancipate himself from the test person's statements and almost without any previous knowledge of the test persons it is possible under certain circumstances to read the most intimate complexes from the results of the experiment. I at first look for the reproduction words and put them together, and I then look for the stimulus words which show the greatest disturbances. In many cases a mere assortment of these words suffices to show the complex. In some cases it is necessary to put a question here and there. It will be best to illustrate this with a concrete example :

It concerns an educated woman of 30 years who has been married for three years. After her marriage she suffers from episodic excitements in which she is violently jealous of her husband. The marriage is a happy one in every other respect and it should be noted that the husband gives no cause for the jealousy. The patient is sure that she loves him and that her excited states are groundless. She cannot imagine whence these excited states originate, and feels quite perplexed over them. It is to be noted that the patient is a catholic and has been brought up religiously, while her husband is a protestant. This difference of religion did not admittedly play any part. A more thorough anamnesis showed the existence of an extreme prudishness. Thus, for example, no one was allowed to talk in the patient's presence about her sister's childbirth, because the sexual moment suggested therein caused her the greatest excitement. She always undressed in the adjoining room and never in her husband's presence, etc. At the age of 27 she was supposed to have had no idea how children were born. The associations gave the results shown in the accompanying chart.

The blue columns represent failures of reproductions, the green ones represent repetitions of stimulus words, and the yellow columns show those associations in which the patient either laughed or made mistakes, using many instead of one word. The height of the columns represent the length of the reaction time.

The stimulus words characterized by marked disturbances are the following: yellow, to pray, to separate, to marry, to quarrel, old, family, happiness, false, fear, to kiss, bride, to choose, contented. The strongest disturbances are found in the following stimulus words: *to pray, to marry, happiness, false, fear, and contented*. These words, therefore, seemingly strike the complex above all. The conclusion that can be drawn from this is that she is not indifferent to the fact that her husband is a protestant, for she again thinks of praying, that there is something wrong with marriage, that she is false, that is, she entertains fancies of faithlessness, she is afraid (of the husband? of the future?), she is not contented with her choice (to choose) and she thinks of separation. The patient therefore has a separation complex for she is very discontented with her married life. When I told her this result she was affected and at first attempted to deny it, then to mince over it, but finally she admitted everything I said and added still more. She reproduced a large number of fancies of faithlessness, reproaches against her husband, etc. *Her prudishness and jealousy were merely a projection of her own sexual wishes on her husband*. Because she was faithless in her fancies and did not admit it to herself she was jealous of her husband.

It is impossible in a lecture to give a review of all the possible uses of the association experiment. I must consent myself with having demonstrated to you at least some of its chief uses.

LECTURE II

THE FAMILIAR CONSTELLATIONS

Ladies and Gentlemen: As you have seen, there are manifold ways in which the association experiment may be employed in practical psychology. I should like to speak to you to-day about another utilization of this experiment which is primarily of only theoretical significance. My pupil, Miss Fürst, M. D., has made the following research: she has applied the association experiment to 24 families, consisting altogether of 100 test persons; the resulting material amounted to 2,200 associations. This material was elaborated in the following manner: Fifteen separate groups were formed according to logical-linguistic standards, and the associations were arranged as follows:

	Husband	Wife	Difference
I. Co-ordination	6.5	0.5	6
II. Sub and supraordination	7	—	7
III. Contrast	—	—	—
IV. Predicate expressing a personal judgment	8.5	95.	86.5
V. Simple predicate	21.	3.5	17.5

	Husband	Wife	Difference
VI. Relations of the verb to the subject or complement	15.5	0.5	15.
VII. Designation of time, etc.	11.	-	11.
VIII. Definition	11.	-	11.
IX. Coexistence	1.5	-	1.5
X. Identity	0.5	0.5	-
XI. Motor-speech combination	12.	-	12
XII. Composition of words	-	-	-
XIII. Completion of words	-	-	-
XIV. Clang associations	-	-	-
XV. Defective reactions	-	-	-
Total,	-	-	173.5
	173.5		
Average difference	15		= 11.5

As can be seen from this example, I utilize the difference to show the degree of the analogy. In order to find a base for the total resemblance I have calculated the differences among all of Miss Fürst's test persons not related among themselves by comparing every female test person with all the other unrelated females; the same has been done for the male test persons.

The most marked difference is found in those cases where the two test persons compared have no associative quality in common. All the groups are calculated in percentages, the greatest difference possible being $\frac{200}{15} = 13.3\%$.

I. The average difference of male unrelated test persons is 5.9%, and that of females of the same group is 6%.

II. The average difference between male related test persons is 4.1%, and that between female related test persons is 3.8%. From these numbers we see that relatives show a tendency to agreement in the reaction type.

III. Difference between fathers and children = 4.2.

" " mothers " " = 3.5.

The reaction types of children come nearer to the type of the mother than to the father.

IV. Difference between fathers and their sons = 3.1.

" " " " " daughters = 4.9.

" " mothers " " sons = 4.7.

" " " " " daughters = 3.0.

V. Difference between brothers = 4.7.

" " sisters = 5.1.

If the married sisters are omitted from the comparison we get the following result:

Difference of unmarried sisters = 3.8.

These observations show distinctly that marriage destroys more or less the original agreement, as the husband belongs to a different type.

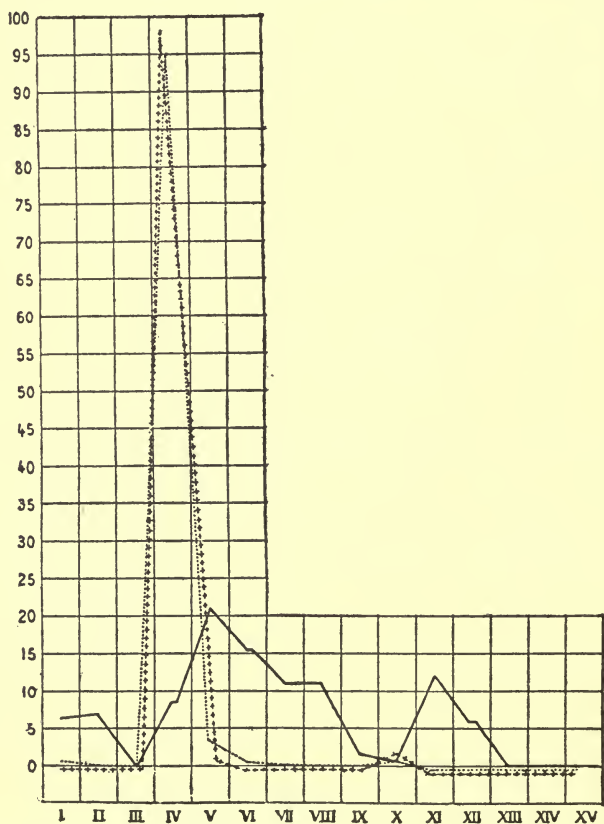
The difference between unmarried brothers = 4.8.

Marriage seems to exert no influence on the association forms in man. Nevertheless, the material which we have at our disposal is not as yet enough to allow us to draw definite conclusions.

VI. The difference between husband and wife = 4.7. This number, however, sums up very inadequately the different values; that is, there are some cases which show a marked difference and some which show a marked agreement.

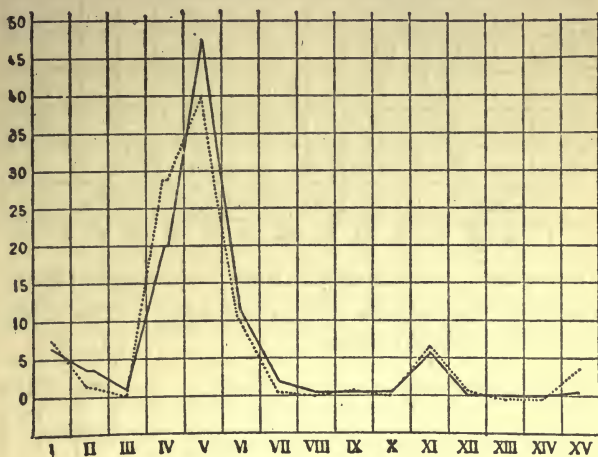
The description in curves of the different results follows.

In the curves here reproduced I have marked above the number of associations of each quality in percentages. The Roman letters written below the diagram designate the forms of association indicated in the above tables (see above).



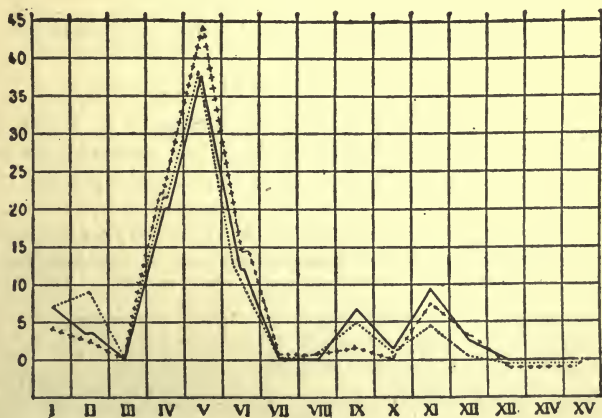
Curve A. — father; mother; ++++ daughter.
 I. Assoc. by co-ordination; II. sub and supraordination; III. contrast, etc. (See above table.)

Curve A. The father (continued line) shows an objective type, while the mother and daughter show the pure predicate type with a pronounced subjective tendency.



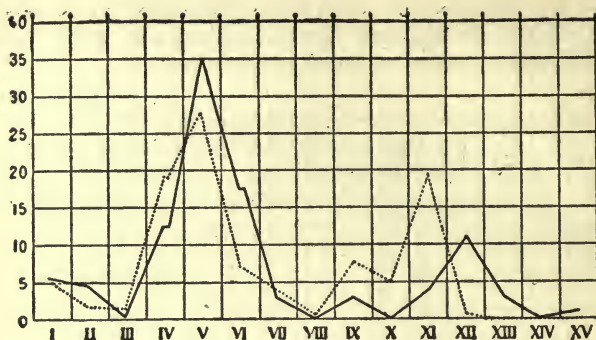
Curve B. — husband; wife.

Curve B. The husband and wife agree well in the predicate objective type, the predicate subjective being somewhat more numerous in the wife.



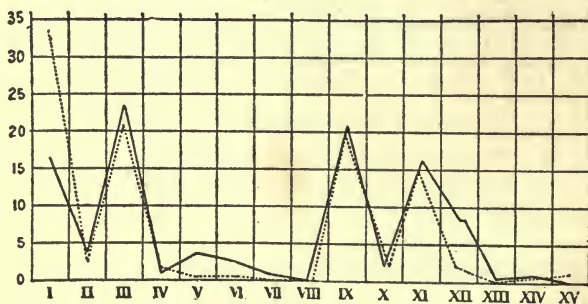
Curve C. — father; 1st daughter; ++++ 2nd daughter.

Curve C. A very nice agreement between a father and his two daughters.



Curve D. — single sister; married sister.

Curve D. Two sisters living together. The dotted line represents the married sister.



Curve E. — husband; wife.

Curve E. Husband and wife. The wife is a sister of the two women of curve D. She approaches very closely to the type of her husband. Her curve is the direct opposite of that of her sisters.

The similarity of the associations is often very extraordinary. I will reproduce here the associations of a mother and her daughter.

Stimulus Word
to pay attention
law
dear
great
potato
family
strange
brother
to kiss

Mother
diligent pupil
command of God
child
God
bulbous root
many persons
traveller
dear to me
mother

Daughter
pupil
Moses
father and mother
father
bulbous root
5 persons
traveller
dear
mother

Stimulus word	Mother	Daughter
burn	great pain	painful
door	wide	big
hay	dry	dry
month	many days	31 days
air	cool	moist
coal	sooty	black
fruit	sweet	sweet
merry	happy child	child

One might indeed think that in this experiment, where full scope is given to chance, individuality would become a factor of the utmost importance, and that therefore one might expect a very great diversity and lawlessness of associations. But as we see the opposite is the case. Thus the daughter lives contently in the same circle of ideas as her mother, not only in her thought but in her form of expression; indeed, she even uses the same words. What seems more flighty, more inconsistent, and more lawless than a fancy, a rapidly passing thought? It is not, however, lawless, and not free, but closely determined within the limits of the milieu. If, therefore, even the superficial and manifestly most flighty formations of the intellect are altogether subject to the milieu-constellation, what should we expect for the more important conditions of the mind, for the emotions, wishes, hopes, and intentions? Let us consider a concrete example,—the curve A. (See above.)

The mother is 45 years old and the daughter 16 years. Both have a very distinct predicate type expressing personal judgment, and differ from the father in the most striking manner. The father is a drunkard and a demoralized creature. We can thus readily understand that his wife perceives an emotional voidness which she naturally betrays by her enhanced predicate type. The same causes cannot, however, operate in the daughter, for in the first place she is not married to a drunkard, and secondly life with all its hopes still lies before her. It is distinctly unnatural for the daughter to show an extreme predicate type expressing personal judgment. She responds to the stimuli of the environment just like her mother. But whereas in the mother the formation is in a way a natural consequence of her unhappy condition of life, this condition is entirely lacking in the daughter. The daughter simply imitates the mother; she merely appears like the mother. Let us consider what this can signify for a young girl. If a young girl reacts to the world like an old woman disappointed in life this at once shows unnaturalness and constraint. But more serious consequences are possible. As you know the predicate type is a manifestation of intensive emotions; emotions are always involved. Thus we cannot prevent ourselves from answering at least inwardly to the feelings and passions of our

nearest environment; we allow ourselves to be infected and carried away by it. Originally the affects and their physical manifestations had a biological significance; *i. e.*, they were a protective mechanism for the individual and the whole herd. If we manifest emotions we can with certainty expect to receive emotions in return. That is the sense of the predicate type. What the 45-year-old woman lacks in emotions; *i. e.*, in love in her marriage relations she seeks to obtain from the outside, and it is for that reason that she is an ardent participant in the Christian Science meetings. If the daughter imitates this situation she does the same thing as her mother, she seeks to obtain emotions from the outside. But for a girl of 16 such an emotional state is to say the least quite dangerous; like her mother she reacts to her environment as a sufferer soliciting sympathy. Such an emotional state is no longer dangerous in the mother, but for obvious reasons it is quite dangerous in the daughter. Once freed from her father and mother she will be like her mother; *i. e.*, she will be a suffering woman craving for inner gratification. She will thus be exposed to the greatest danger of falling a victim to brutality and of marrying a brute and inebriate like her father.

This consideration seems to me to be of importance for the conception of the influence of environment and education. The example shows what passes over from the mother to the child. It is not the good and pious precepts, nor is it any other inculcation of pedagogic truths that have a moulding influence upon the character of the developing child, but what most influences him is the peculiarly affective state which is totally unknown to his parents and educators. The concealed discord between the parents, the secret worry, the repressed hidden wishes, all these produce in the individual a certain affective state with its objective signs which slowly but surely, though unconsciously, works its way into the child's mind, producing therein the same conditions and hence the same reactions to external stimuli. We know that association with mournful and melancholic persons will depress us, too. A restless and nervous individual infects his surroundings with unrest and dissatisfaction, a grumbler, with his discontent, etc. If grown-up persons are so sensitive to such surrounding influences we certainly ought to expect more of this in the child whose mind is as soft and plastic as wax. The father and mother impress deeply into the child's mind the seal of their personality, the more sensitive and mouldable the child the deeper is the impression. Thus even things that are never spoken about are reflected in the child. The child imitates the gesture, and just as the gesture of the parent is the expression of an emotional state, so in turn the gesture gradually produces in the child a

similar feeling, as it feels itself, so to speak, into the gesture. Just as the parents adapt themselves to the world so does the child. At the age of puberty when it begins to free itself from the spell of the family, it enters into life with so to say a surface of fracture entirely in keeping with that of the father and mother. The frequent and often very deep *depressions of puberty* emanate from this; they are symptoms which are rooted in the difficulty of new adjustment. The youthful person at first tries to separate himself as much as possible from his family, he may even estrange himself from it, but inwardly this only ties him the more firmly to the parental image. I recall the case of a young neurotic who ran away from his parents, he was strange and almost hostile to them, but he admitted to me that he possessed a special sanctum; it was a strong box containing his old childhood books, old dried flowers, stones, and even small bottles of water from the well at his home and from a river along which he walked with his parents, etc.

The first attempts to assume friendship and love are constellated in the strongest manner possible by the relation to parents, and here one can usually observe how powerful are the influences of the familiar constellations. It is not rare, *e. g.*, for a healthy man whose mother was hysterical to marry a hysterical, or for the daughter of an alcoholic to choose an alcoholic for her husband. I was once consulted by an intelligent and educated young woman of 26 who suffered from a peculiar symptom. She thought that her eyes now and then took on a strange expression which exerted a disagreeable influence on men. If she then looked at a gentleman he became embarrassed, turned away and said something rapidly to his neighbor, at which both were either embarrassed or inclined to laugh. The patient was convinced that her look excited indecent thoughts in the men. It was impossible to convince her of the falsity of her conviction. This symptom immediately aroused in me the suspicion that I dealt with a case of paranoia rather than with a neurosis. But as was shown only three days later by the further course of the treatment, I was mistaken, for the symptom promptly disappeared after it had been explained by analysis. It originated in the following manner: The lady had a lover who deserted her in a very striking manner. She felt utterly forsaken, she withdrew from all society and pleasure, and entertained suicidal ideas. In her seclusion there accumulated unadmitted and repressed erotic wishes which she unconsciously projected on men whenever she was in their company. This gave rise to her conviction that her look excited erotic wishes in men. Further investigation showed that her deserting lover was *alunatic*, which she did not apparently observe. I expressed my surprise at her unsuitable choice and added

that she must have had a certain predilection for loving mentally abnormal persons. This she denied, stating that she had once before been engaged to be married to a normal man. He, too, deserted her; and on further investigation it was found that he, too, had been in an insane asylum shortly before,—another lunatic! This seemed to me to confirm with sufficient certainty my belief that she had an unconscious tendency to choose insane persons. Whence originated this strange taste? Her father was an eccentric character, and in later years entirely estranged from his family. Her whole love had therefore been turned away from her father to a brother 8 years her senior; him she loved and honored as a father, and this brother became hopelessly insane at the age of 14. That was apparently the model from which the patient could never free herself, after which she chose her lovers, and through which she had to become unhappy. Her neurosis which gave the impression of insanity probably originated from this infantile model. We must take into consideration that we are dealing in this case with a highly educated and intelligent lady who did not pass carelessly over her mental experiences, who indeed reflected much over her unhappiness without, however, having any idea whence her misfortune originated.

These are things which inwardly appeal to us as matter of course, and it is for this reason that we do not see them but attribute everything to the so-called congenital character. I could cite any number of examples of this kind. Every patient furnishes contributions to this subject of the determination of destiny through the influence of the familiar milieus. In every neurotic we see how the constellation of the infantile milieu influences not only the character of the neurosis but also life's destiny, in its very details. Numberless unhappy choices of profession and matrimonial failures can be traced to this constellation. There are, however, cases where the profession has been happily chosen, where the husband or wife leaves nothing to be desired, and where still the person does not feel well but works and lives under constant difficulties. Such cases often appear in the guise of chronic neurasthenia. Here the difficulty is due to the fact that the mind is unconsciously split into two parts of divergent tendencies which are impeding each other; one part lives with the husband or with the profession, while the other lives unconsciously in the past with the father or mother. I have treated a lady who, after suffering many years from a severe neurosis, merged into a dementia præcox. The neurotic affection began with her marriage. This lady's husband was kind, educated, well to do, and in every respect suitable for her; his character showed nothing that would in any way in-

terfere with a happy marriage. Despite that the marriage was an unhappy one merely because the wife was neurotic and therefore prevented all congenial companionship.

The important heuristic axiom of every psychoanalysis reads as follows: *If a neurosis springs up in a person this neurosis contains the counter-argument against the relationship of the patient to the personality with which he is most intimately connected.* If the husband has a neurosis the neurosis thus loudly proclaims that he has intensive resistances and contrary tendencies against his wife, and if the wife has a neurosis the wife has a tendency which diverges from her husband. If the person is unmarried the neurosis is then directed against the lover or the sweetheart or against the parents. Every neurotic naturally strives against this relentless formulation of the content of his neurosis, and he often refuses to recognize it at any cost, but still it is always justified. To be sure the conflict is not on the surface but must generally be revealed through a painstaking psychoanalysis.

The history of our patient reads as follows :

The father had a powerful personality. She was his favorite daughter and entertained for him a boundless veneration. At the age of 17 she for the first time fell in love with a young man. At that time she had twice the same dream, the impression of which never left her in all her later years ; she even imputed to it a mystic significance and often recalled it with religious dread. In the dream she saw a tall, masculine figure with a very beautiful white beard ; at this sight she was permeated with a feeling of awe and delight as if she experienced the presence of God himself. This dream made the deepest impression on her, and she was constrained to think of it again and again. The love affair of that period proved to be one of little warmth and was soon given up. Later the patient married her present husband. Though she loved her husband she was led continually to compare him with her deceased father ; this comparison always proved unfavorable to her husband. Whatever the husband said, intended, or did, was subjected to this standard and always with the same result : " My father would have done all this better and differently." Our patient's life with her husband was not happy, she could neither respect nor love him sufficiently ; she was inwardly dissatisfied and unsatiated. She gradually evinced a fervent piety, and at the same time there appeared a violent hysterical affection. She began by going into raptures now over this and now over that clergyman, she was looking everywhere for a spiritual friend, and estranged herself more and more from her husband. The mental trouble made itself manifest after about a decade. In her diseased state she refused to have

anything to do with her husband and child; she imagined herself pregnant by another man. In brief, the resistances against her husband which hitherto had been laboriously repressed came out quite openly, and among other things manifested themselves in insults of the gravest kind directed against her husband.

In this case we see how a neurosis appeared, as it were at the moment of marriage, *i. e.*, *this neurosis expresses the counter-argument against the husband*. What is the counter-argument? The counter-argument is the father of the patient, for she verified daily her belief that her husband was not equal to her father. When the patient first fell in love there also appeared a symptom in the form of a very impressive visionary dream. She saw the man with the very beautiful white beard. Who was this man? On directing her attention to the beautiful white beard she immediately recognized the phantom. It was of course her father. Thus every time the patient merged into a love affair the picture of the father inopportunely appeared and prevented her from adjusting herself psychologically to her husband.

I purposely chose this case as an illustration because it is simple, obvious, and quite typical of many marriages which are crippled through the neurosis of the wife. The unhappiness always lies in a too firm attachment to the parents. The offspring remains in the infantile relations. We can find here one of the most important tasks of pedagogy, namely, the solution of the problem how to free the growing individual from his unconscious attachments to the influences of the infantile milieu, in such a manner that he may retain whatever there is in it that is suitable and reject whatever is unsuitable. To solve this difficult question on the part of the child seems to me impossible at present. We know as yet too little about the child's emotional processes. The first and only real contribution to the literature on this subject has in fact appeared during the present year. It is the analysis of a five-year-old boy published by Freud.

The difficulties on the part of the child are very great. They should not, however, be so great on the part of the parents. In many ways the parents could manage more carefully and more indulgently the love of children. The sins committed against favorite children by the undue love of the parents could perhaps be avoided through a wider knowledge of the child's mind. For many reasons I find it impossible to tell you anything of general validity concerning the bringing up of children as it is affected by this problem. We are as yet very far from general prescriptions and rules; are still in the realm of casuistry. Unfortunately our knowledge of the finer

mental processes in the child is so meagre that we are after all not in any position to say where the greater trouble lies, whether in the parents, in the child, or in the conception of the milieu. Only psychanalyses of the kind that Professor Freud has published in our *Jahrbuch*, 1909, will help us out of this difficulty. Such comprehensive and profound observations should act as a strong inducement to all teachers to occupy themselves with Freud's psychology. This psychology offers more for practical pedagogy than the physiological psychology of the present.

LECTURE III

EXPERIENCES CONCERNING THE PSYCHIC LIFE OF THE CHILD

Ladies and Gentlemen: In the last lecture we have seen how important for later life are the emotional processes of childhood. In to-day's lecture I should like to give you some insight into the psychic life of the child through the analysis of a 4-year-old girl. It is much to be regretted that there are doubtless few among you who have had opportunity to read the analysis of "Little John" (*Kleiner Hans*), which has been published by Freud during the current year.¹ I should properly begin by giving you the content of that analysis, so that you might be in a position to compare for yourselves the results of Freud with those obtained by me, and to observe the marked, even astonishing, similarity between the unconscious creations of the two children. Without a knowledge of the fundamental analysis of Freud, much in the report of the following case will appear to you strange, incomprehensible, and perhaps unacceptable. I beg you, however, to defer final judgment and to enter upon the consideration of these new subjects with a kindly disposition, for such pioneer work in virgin soil requires not only the greatest patience on the part of the investigator, but also the unprejudiced attention of his audience. Because the Freudian investigations apparently involve an indelicate discussion of the most intimate secrets of sexuality many people have had a feeling of repulsion and have therefore rejected everything as a matter of course without any real proof. This, unfortunately, has almost always been the fate of Freud's doctrines until now. One must not come to the consideration of these matters with the firm conviction that they do not exist, else it may easily come to pass that for the prejudiced they really do not exist. One should perhaps for the moment assume the author's point of view and investigate these phenomena under his guidance. In this way only can

¹Jahrbuch f. Psychoanalytische und Psychopathologische Forschungen, Band I, Deuticke, Wien.

the correctness or incorrectness of our observations be affirmed. We may err, as all human beings err. But the continual holding up to us of our mistakes,—perhaps they are worse than mistakes,—does not help us to see things more distinctly. We should prefer to see *wherein* we err. That should be shown to us in our own sphere of experience. Thus far, however, no one has succeeded in meeting us on our own ground, and in giving us a different conception of the things which we ourselves see. We must still complain that our critics are persisting in complete ignorance and without the slightest notion about the matters in question. The only reason for this is that our critics have never taken the trouble to become thoroughly acquainted with our method; had they done this they would have understood us.

The little girl to whose sagacity and intellectual vivacity we are indebted for the following observations is a healthy, lively child of emotional temperament. She has never been seriously ill, and never, even in the realm of the nervous system, had there been observed any symptoms prior to this investigation. In the report which will now follow we shall have to waive a connected description, for it is made up of anecdotes which treat of one out of a whole cycle of similar experiences, and which cannot, therefore, be arranged scientifically and systematically, but must rather be described somewhat in the form of a story. This manner of description we cannot as yet dispense with in our analytic psychology, for we are still far from being able in all cases to separate with unerring certainty the curious from the typical.

When the little daughter, whom we will call Anna, was about 3 years old, she once had the following conversation with her grandmother:

Anna: "Grandma, why have you such withered eyes?"

Grandma: "Because I am old?"

A. "But you will become young again."

G. "No, do you know, I shall become older and older, and then I shall die."

A. "Well, and then?"

G. "Then I shall become an angel."

A. "And then will you again become a little child?"

The child found here a welcome opportunity for the provisional solution of a problem. For some time before she had been in the habit of asking her mother whether she would ever have a living doll, a little child, a little brother. This naturally included the question as to the origin of children. As such questions appeared only spontaneously and indirectly, the parents attached no significance to them, but received

them as lightly and in appearance as facetiously as the child seemed to ask them. Thus she once received from her father the amusing information that children are brought by the stork. Anna had already heard somewhere a more serious version, namely, that children are little angels living in heaven and are brought from heaven by the stork. This theory seems to have become the starting point for the investigating activity of the little one. From the conversation with the grandmother it could be seen that this theory was capable of wide application, namely, it not only solved in a comforting manner the painful idea of parting and dying, but at the same time it solved satisfactorily the riddle of the origin of children. Such solutions which kill at least two birds with one stone were formerly tenaciously adhered to in science, and even in the child they cannot be made retrograde without some shock.

Just as was the birth of a little sister the turning point in the history of "little John," so it was in this case the birth of a brother, which happened when Anna had reached the age of 4 years. The pregnancy of the mother apparently remained unnoticed; *i. e.*, the child never expressed herself on this subject. On the evening before the childbirth when the labor pains began to manifest themselves in the mother, the child was in her father's room. He took her on his knee and said, "Tell me, what would you say if you should get a little brother to-night?" "I would kill it," was the prompt answer. The expression "to kill" looks very serious, but in reality it is quite harmless, for "to kill" and "to die" in child language signify only to remove either in the active or in the passive sense, as has already been pointed out a number of times by Freud. "To kill" as used by the child is a harmless word, especially so when we know that the child uses the word "kill" quite promiscuously for all possible kinds of destruction, removal, demolition, etc. It is, nevertheless, worth while to note this tendency (see the analysis of *Kleiner Hans*, p. 5).

The childbirth occurred early in the morning in the presence of a physician and a midwife. When all remnants of the birth, including some blood traces, were cleaned up, the father entered the room where the little one slept. She awoke as he entered. He imparted to her the news of the advent of a little brother which she took with surprise and strained facial expression. The father took her in his arms and carried her into the confinement chamber. She first threw a rapid glance at her somewhat pale mother and then displayed something like a mixture of despair and suspicion as if thinking, "Now what else is going to happen? (Father's impression.) She displayed hardly any pleasure at the sight of the new arrival, so that the cool reception she gave it caused general disappointment.

During the forenoon she kept very noticeably away from her mother; this was the more striking as she was usually much attached to her mother. But once when her mother was alone she ran into the room, embraced her and said, "Well, are n't you going to die now?" This explains a part of the conflict in the child's psyche. Though the stork theory was never really taken seriously, she accepted the fruitful re-birth hypothesis, according to which a person by dying assisted a child into life. Accordingly the mother, too, must die; why, then, should the newborn child, against whom she already felt childish jealousy, cause her pleasure? It was for this reason that she had to ascertain in a favorable moment whether the mother was to die, or rather was moved to express the hope that she would not die.

With this happy issue, however, the re-birth theory sustained a severe shock. How was it possible now to explain the birth of her little brother and the origin of children in general? There still remained the stork theory which, though never expressly rejected, had been implicitly waived through the assumption of the re-birth theory. The explanations next attempted unfortunately remained hidden from the parents as the child stayed a few weeks with her grandmother. From the grandmother's report we learned that the stork theory was often discussed, and it was naturally re-enforced by the concurrence of those about her.

When Anna returned to her parents she again on meeting her mother evinced the same mixture of despair and suspicion which she had displayed after the birth. The impression, though inexplicable, was quite unmistakable to both parents. Her behavior towards the baby was very nice. During her absence a nurse had come into the house who, on account of her uniform made a deep impression on Anna; to be sure, the impression at first was quite unfavorable as she evinced the greatest hostility to her. Thus nothing could induce her to allow herself to be undressed and put to sleep by this nurse. Whence this resistance originated was soon shown in an angry scene near the cradle of the little brother in which Anna shouted at the nurse, "This is not your little brother, it is mine!" Gradually, however, she became reconciled to the nurse and began to play nurse herself, she had to have her white cap and apron and "nursed" now her little brother and now her doll.

In contrast to her former mood she became unmistakably mournful and dreamy. She often sat for a long time under the table singing and rhyming stories which were partially incomprehensible but sometimes contained the "nurse" theme ("I am a nurse of the green cross"). Some of the stories, how-

ever, distinctly showed a painful feeling striving for expression.

Here we meet with a new and important feature in the little one's life, that is, we meet with reveries, tendencies towards the composition of poetry, and melancholic attacks. All these things which we are wont first to encounter at a later period of life, at a time when the youthful person is preparing to sever the family tie and to enter independently upon life, but is still held back by an inward, painful feeling of homesickness and the warmth of the parental hearth. At that time the youth begins to replace his longing with poetic fancies in order to compensate for the deficiency. To approximate the psychology of a four-year-old child to that of the age of puberty will at first sight seem paradoxical, the relationship lies, however, not in the age but rather in the mechanism. The elegiac reveries express the fact that a part of that love which formerly belonged and should belong to a real object is now *introverted*, that is, it is turned inward into the subject and there produces an increased imaginative activity. What is the origin of this *introversion*? Is it a psychological manifestation peculiar to this age, or does it owe its origin to a conflict?

This is explained in the following occurrence. It often happened that Anna was disobedient to her mother, she was insolent, saying, "I am going back to grandma."

Mother: "But I shall be sad when you leave me."

Anna: "Oh, but you have the little brother."

The effect which this produced on the mother shows what the little one was really aiming at with her threats to go away again; she apparently wished to hear what her mother would say to her proposal, that is, to see what attitude her mother would actually assume to her, whether her little brother had not crowded her out altogether from her mother's favor. One must, however, give no credence to this little trickster. For the child could readily see and feel that despite the existence of the little brother there was nothing essentially lacking for her in her mother's love. The reproach to which she subjects her mother is therefore unjustified and to the trained ear this is betrayed by a slightly affected tone. Such a tone if unmistakable, shows that it does not expect to be taken seriously and hence it obtrudes itself re-enforced. The reproach as such must also not have been taken seriously by the mother for it was only the forerunner of other and this time more serious resistances. Not long after the previously reported conversation the following scene took place:

Mother: "Come, we are going into the garden now!"

Anna: "You are lying, take care if you are not telling the truth."

M. "What are you thinking of? I always tell the truth."

A. "No, you are not telling the truth."

M. "You will soon see that I am telling the truth; we are going into the garden now."

A. "Indeed, is that true? Is that really true? Are you not lying?"

Scenes of this kind were repeated a number of times. This time the tone was more rude and more penetrating, and at the same time the accent on the word "lie" betrayed something special which the parents did not understand; indeed, at first they attributed too little significance to the spontaneous utterances of the child. In this they merely did what education usually does with official sanction. One usually pays little heed to children in every stage of life; in all essential matters, they are treated as not responsible, and in all unessential matters, they are trained with an automatic precision.

Under resistances there always lies a question, a conflict, of which we hear at later times and on other occasions. But usually one forgets to connect the thing heard with the resistances. Thus, on another occasion Anna put to her mother the following difficult questions:

Anna: "I should like to become a nurse when I grow big, —why did you not become a nurse?"

Mother: "Why, as I have become a mother I have children to nurse anyway."

A. (Reflecting) "Indeed, shall I be a different woman from you, and shall I still speak to you?"

The mother's answer again shows whither the child's question was really directed. Apparently Anna, too, would like to have a child to "nurse" just as the nurse has. Where the nurse got the little child is quite clear. Anna, too, could get a child in the same way if she were big. Why did not the mother become such a plain nurse, that is to say, how did she get a child if not in the same way as the nurse? Like the nurse, Anna, too, could get a child, but how that fact might be changed in the future or how she might come to resemble her mother in respect to getting children is not clear to her. From this resulted the thoughtful question, "Indeed, shall I be a different woman from you? Shall I be different in every respect?" The stork theory evidently had come to naught, the dying theory met a similar fate; hence she now thinks one may get a child in the same way, as, for example, the nurse got hers. She, too, could get one in this natural way, but how about the mother who is no nurse and still has children? Looking at the matter at this point of view, Anna asks: "Why did you not become a nurse?" namely, "why have you not got your child in the natural way?" This peculiar indirect

manner of questioning is typical, and evidently corresponds with the child's hazy grasp of the problem, unless we assume a certain diplomatic uncertainty prompted by a desire to evade direct questioning. We shall later find an illustration of this possibility. Anna is evidently confronted with the question "where does the child come from?" The stork did not bring it; mother did not die; nor did mother get it in the same way as the nurse. She has, however, asked this question before and received the information from her father that the stork brings children; this is positively untrue, she can never be deceived on this point. Accordingly, papa and mama and all the others lie. This readily explains her suspicion at the childbirth and her discrediting of her mother. But it also explains another point, namely, the elegiac reveries which we have attributed to a partial introversion. We know now from what real object love had to be taken and introverted to no purpose, namely, it had to be taken *from the parents* who deceived her and refused to tell her the truth. (What must this be which cannot be uttered? What is going on here?) Such were the parenthetical questions of the child, and the answer was: Evidently this must be something to be concealed, perhaps something dangerous. Attempts to make her talk and to draw out the truth by means of (insidious) questions were futile, she exerted *resistance against resistance*, and the introversion of love began. It is evident that the capacity for sublimation in a 4-year-old child is still too slightly developed to be capable of performing more than symptomatic services. The mind, therefore, depends on another compensation, namely, it resorts to one of the relinquished infantile devices for securing love by force, the most preferred is that of crying and calling the mother at night. This has been diligently practised and exhausted during her first year. It now returns and corresponding to the period of life it has become well determined and equipped with recent impressions. It was just after the earthquakes in Messina, and this event was discussed at the table. Anna was extremely interested in everything, she repeatedly asked her grandma to relate to her how the earth shook, how the houses were demolished and many people lost their lives. After this she had nocturnal fears, she could not remain alone, her mother was forced to go to her and stay with her; otherwise she feared that an earthquake would appear, that the house would fall and kill her. During the day, too, she was much occupied with such thoughts. While walking with her mother she annoyed her with such questions as, "Will the house be standing when we return home? Are you sure there is no earthquake at home? Will papa still be living?" About every stone lying in the road she asked whether it was from an

earthquake. A new building was a house destroyed by the earthquake, etc. She finally even cried out frequently at night that the earthquake was coming and that she heard the thunder. In the evening she had to be solemnly assured that there was no earthquake coming.

Many means of calming her were tried, thus she was told, for example, that earthquakes only exist where there are volcanoes. But then she had to be satisfied that the mountains surrounding the city were not volcanoes. This reasoning gradually caused in the child an eager desire for learning, strong but quite unnatural for her age, which manifested itself in her requiring that all the geological atlases and text-books should be brought her from her father's library. For hours she rummaged through these works looking for pictures of volcanoes and earthquakes, and asking questions continually. We are here confronted by an energetic effort to sublimate the fear into an eager desire for learning, which at this age made a decidedly premature exaction; but, as in many a gifted child which suffers from precisely the same difficulty, many effects of this immature sublimation were surely not to her advantage. For, by favoring sublimation at this age one merely enforces a fragment of neurosis. The root of the eager desire for learning is the *fear* and the *fear is the expression of a converted libido*; that is, it is the expression of *an introversion which henceforth becomes neurotic*, which at this age is neither necessary nor favorable for the development of the child.

Whither this eager desire for learning was ultimately directed is explained by a series of questions which arose almost daily. "Why is Sophie (a younger sister) younger than I?" "Where was Freddy (the little brother) before? Was he in heaven? What was he doing there? Why did he come down just now, why not before?"

This state of affairs induced the father to decide that the mother should tell the child when occasion offered *the truth concerning the origin of the little brother*. This having been done Anna soon thereafter asked about the stork. Her mother told her that the story of the stork was not true, but that Freddy grew up in his mother like the flowers in a plant. At first he was very little, and then he became bigger and bigger just like a plant. She listened attentively without the slightest surprise, and then asked, "But did he come out all by himself?"

Mother: "Yes."

Anna: "But he cannot walk!"

Sophie: "Then he crawled out."

Anna, overhearing her little sister's answer,— "Is there a hole here? (pointing to the breast) or did he come out of the mouth? Who came out of the nurse?" She then interrupted

herself and exclaimed, "No, no, the stork brought little brother down from heaven." She soon left the subject and again wished to see pictures of volcanoes. During the evening following this conversation she was calm. The sudden explanation produced in the child a whole series of ideas, which manifested themselves in certain questions. Unexpected perspectives were opened; she rapidly approached the main problem, namely, the question, "*Where did the child come out?*" *Was it from a hole in the breast or from the mouth?* Both suppositions are entirely qualified to form acceptable theories. We even meet with recently married women who still entertain the theory of the hole in the abdominal wall or of the Cæsarean section; this is supposed to betray a very curious form of innocence. But as a matter of fact it is not innocence, as we are always dealing in such cases with infantile sexual activities, which in later life have brought the *vias naturales* into ill repute.

It may be asked where the child got the absurd idea that there is a hole in the breast, or that the birth takes place through the mouth. Why did she not select one of the natural openings existing in the abdomen from which things come out daily? The explanation is simple. Very shortly before, our little one had invited some educational criticism on her mother's part by a heightened interest in both abdominal openings with their remarkable products,—an interest not always in accord with the requirements of cleanliness and decorum. Then for the first time she became acquainted with the exceptional laws of these bodily regions and, being a sensitive child, she soon learned that there was something here to be tabooed. This region, therefore, must not be referred to. Anna had simply shown herself docile and had so adjusted herself to the cultural demands that she thought (at least spoke) of the simplest things last. The incorrect theories substituted for correct laws persisted for years until brusque explanations came from without. It is, therefore, no wonder that such theories, the forming of and adherence to which are favored even by parents and educators should later become determinants of important symptoms in a neurosis, or of delusions in a psychosis, just as I have shown that in dementia præcox¹ what has existed in the mind for years always remains somewhere, though it may be hidden under compensations seemingly of a different kind.

But even before this question, whence the child really comes out, was settled, a new problem obtruded itself; viz., the children come out of the mother, but how is it with the nurse?

¹Jung: The Psychology of Dementia Præcox, translated by Peterson and Brill. *Journal of Nervous and Mental Diseases*, Monograph Series, No. 3.

Did some one come out also in this case? This question was followed by the remark, "No, no, the stork brought down the little brother from heaven." What is there peculiar about the fact that nobody came out of the nurse? We recall that Anna identified herself with the nurse and planned to become a nurse later, for,—she, too, would like to have a child, and she could have one as well as the nurse. But now when it is known that the little brother grew in mama, how is it now?

This disquieting question is averted by a quick return to the stork-angel theory which has never been really believed and which after a few trials is at last definitely abandoned. Two questions, however, remain in the air. The first reads as follows: Where does the child come out? The second, a considerably more difficult one, reads: How does it happen that mama has children while the nurse and the servants do not? All these questions did not at first manifest themselves.

On the day following the explanation while at dinner, Anna spontaneously remarked: "My brother is in Italy, and has a house of cloth and glass, but it does not tumble down."

In this case as in the others it was impossible to ask for an explanation; the resistances were too great and Anna could not be drawn into conversation. This former, officious and pretty explanation is very significant. For some three months the two sisters had been building a stereotyped fanciful conception of a "big brother." This brother knows everything, he can do and has everything, he has been and is in every place where the children are not; he is owner of great cows, oxen, horses, dogs; everything is his, etc. Each sister has such a "big brother." We must not look far for the origin of this fancy; the model for it is the *father* who seems to correspond to this conception: he seems to be like a brother to mama. The children, too, have their similar powerful "brother." This brother is very brave; he is at present in dangerous Italy and inhabits an impossible fragile house, and *it does not tumble down*. For the child this realizes an important wish. *The earthquake is no longer to be dangerous*. As a consequence of this the child's fear disappeared and *stayed away*. The fear of earthquakes now entirely vanished. Instead of calling her father to her bed to conjure away the fear, she now became very affectionate and begged him every night to kiss her.

In order to test this new state of affairs the father showed her pictures illustrating volcanoes and earthquake devastations. Anna remained unaffected, she examined the pictures with indifference, remarking, "These people are dead; I have already seen that quite often." The picture of a volcanic eruption no longer had any attraction for her. Thus all her scientific interest collapsed and vanished as suddenly as it came. During

the days following the explanation Anna had quite important matters to occupy herself with; she disseminated her newly acquired knowledge among those about her in the following manner: She began by again circumstantially affirming what had been told her, viz., that Freddy, she, and her younger sister had grown in her mother, that papa and mama grew in their mothers, and that the servants likewise grew in their respective mothers. By frequent questions she tested the true basis of her knowledge, for her suspicion was aroused in no small measure, so that it needed many confirmations to remove all her uncertainties.

On one occasion the trustworthiness of the theory threatened to go to pieces. About a week after the explanation the father was taken sick with influenza and consequently had to remain in bed during the forenoon. The children knew nothing about this, and Anna coming into the parents' bedroom saw what was quite unusual, namely, that her father was remaining in bed. She again took on a peculiar surprised expression; she remained at a distance from the bed and would not come nearer; she was apparently again reserved and suspicious. But suddenly she burst out with the question, "Why are you in bed, have you a plant in your belly, too?"

The father was naturally forced to laugh. He calmed her, however, by assuring her that children never grow in the father, that only women can have children and not men; thereupon the child again became friendly. But though the surface was calm the problems continued to work in the dark. A few days later while at dinner Anna related the following dream: "I dreamed last night of Noah's ark." The father then asked her what she had dreamed about it, but Anna's answer was sheer nonsense. In such cases it is necessary only to wait and pay attention. A few minutes later she said to her mother, "I dreamed last night about Noah's ark, and there were a lot of little animals in it." Another pause. She then began her story for the third time. "*I dreamed last night about Noah's ark, and there were a lot of little animals in it, and underneath there was a lid and that opened and all the little animals fell out.*"

The children really had a Noah's ark, but its opening, a lid, was on the roof and not underneath. In this way she delicately intimated that the story of the birth from mouth or breast is incorrect, and that she had some inkling where the children came out.

A few weeks then passed without any noteworthy occurrences. On one occasion she related the following dream: "I dreamed about papa and mama; they had been sitting late in the study and we children were there too." On the face of

this we find a wish of the children, to be allowed to sit up as long as the parents. This wish is here realized or rather it is utilized to express a more important wish, namely, *to be present in the evening when the parents are alone*; of course quite innocently it was in the *study* where she has seen all the interesting books and where she has satiated her thirst for knowledge; *i. e.*, she was really seeking an answer to the burning question, whence the little brother came. If the children were there they would find out.¹ A few days later Anna had a terrifying dream from which she awoke crying, "The earthquake was coming, the house had begun to shake." Her mother went to her and calmed her by saying that the earthquake was not coming, that everything was quiet, and that everybody was asleep. Whereupon Anna said: "*I would like to see the spring, when all the little flowers are coming out and the whole lawn is full of flowers—I would like to see Freddy, he has such a dear little face—What is papa doing? What is he saying?*" (The mother said, "He is asleep and is n't saying anything now.") Little Anna then remarked with a sarcastic smile: "*He will surely be sick again in the morning.*"

This text should be read backwards. The last sentence was not meant seriously, as it was uttered in a mocking tone. When the father was sick the last time Anna suspected that he had a "plant in his belly." The sarcasm signifies: "To-morrow papa is surely going to have a child." But this also is not meant seriously. Papa is not going to have a child; mama alone has children; perhaps she will have another child to-morrow; but where from? "What does papa do?" The formulation of the difficult problem seems here to come to the surface. It reads: What does papa really do if he does not bear children? The little one is very anxious to have a solution for all these problems, she would like to know how Freddy came into the world, she would like to see how the little flowers come out of the earth in the spring, and these wishes are hidden behind the fear of earthquakes.

After this intermezzo Anna slept quietly until morning. In the morning her mother asked her what she had dreamed. She did not at first recall anything, and then said: "*I dreamed that I could make the summer, and then some one threw a Punch² down into the closet.*"

This peculiar dream apparently has two different scenes which are separated by "then." The second part draws its material from the recent wish to possess a Punch, that is, to

¹ This wish to sit up with the father and mother until late at night often plays a great part later in a neurosis. Its object is to prevent the parental coitus.

² A doll from Punch and Judy.

have a masculine doll just as the mother has a little boy. Some one threw Punch down into the closet; one often lets other things fall down into the water closet. *It is just like this that the children, too, come out.* We have here an analogy to the "Lumpf-theory" of little John.¹ Whenever several scenes are found in one dream, each scene ordinarily represents a particular variation of the complex elaboration. Here accordingly the first part is only a variation of the theme found in the second part. The meaning of "to see the spring" or "to see the little flowers come out" we have already seen. Anna now dreams that she *can make the summer*, that is she can bring it about that the little flowers shall come out. She herself can make a little child, and the second part of the dream represents this just like a passage of the bowels. Here we find the egotistic wish which is behind the seemingly objective interest of the nocturnal conversation.

A few days later the mother was visited by a lady who expected soon to become a mother. The children seemed to take no interest in the matter, but the next day they amused themselves with the following play which was directed by the older one: they took all the newspapers they could find in their father's paperbasket and stuffed them under their clothes, so that the intention of the imitation was quite plain. During the night little Anna had another dream: "*I dreamed about a woman in the city, she had a very big belly.*" The chief actor in the dream is always the dreamer himself under some definite aspect; thus the childish play of the day before is fully solved.

Not long thereafter Anna surprised her mother with the following performance: She stuck her doll under her clothes, then pulled it out slowly head downwards, and at the same time remarked, "*Look, the little child is coming out, it is now all out.*" By this means Anna tells her mother, "You see, thus I apprehend the problem of birth. What do you think of it? Is that right?" The play is really meant to be a question, for, as we shall see later, this conception had to be officially confirmed. That rumination on this problem by no means ended here is shown by the occasional ideas conceived during the following weeks. Thus she repeated the same play a few days later with her Teddy Bear, which functioned as an especially loving doll. One day, looking at a rose, she said to her grandma, "See, the rose is getting a baby." As her grandma did not quite understand her she pointed to the enlarged calyx and said, "You see she is quite thick here."

Anna once quarrelled with her younger sister, and the latter

¹See analysis of a 5-year-old boy, *Jahrbuch f. Psychoanalytische u. Psychopathologische Forschungen*, Vol. I.

angrily exclaimed, "I will kill you." Whereupon Anna answered, "When I am dead you will be all alone; then you will have to pray to the dear Lord for a live baby." But the scene soon changed: Anna was the angel, and the younger sister was forced to kneel before her and pray to her that she should present to her a living child. In this way Anna became the presenting mother.

Oranges were once served on the table. Anna impatiently asked for one and said, "*I am going to take an orange and swallow it all down into my belly, and then I shall get a little child.*" Who will not think here of the fairy tales in which childless women finally become pregnant by swallowing fruit, fish, and similar things.¹ Thus Anna attempts to solve the problem *how the children actually come into the mother*. She thus enters into an examination which hitherto has not been formulated with so much sharpness. The solution follows in the form of an *analogy*, which is quite characteristic of the archaic thinking of the child. (In the adult, too, there is a kind of thinking by analogy which belongs to the stratum lying immediately below consciousness. Dreams bring the analogies to the surface; the same may be observed also in dementia præcox.) In German as well as in numerous foreign fairy tales one frequently finds such characteristic childish comparisons. Fairy tales seem to be the myths of the child, and therefore contain among other things the mythology which the child weaves concerning the sexual processes. The spell of the fairy tale poetry, which is felt even by the adult, is explained by the fact that some of the old theories are still alive in our unconscious minds. We experience a strange, peculiar and familiar feeling when a conception of our remotest youth is again stimulated. Without becoming conscious it merely sends into consciousness a feeble copy of its original emotional strength.

The problem how the child gets into the mother was difficult to solve. As the only way of taking things into the body is through the mouth, it could evidently be assumed that the mother eats something like a fruit which then grows in her belly. But then comes another difficulty, namely, it is clear enough what the mother produces but it is not yet clear what the father is good for.

What does the father do? Anna now occupied herself exclusively with this question. One morning she ran into the parents' bedroom while they were dressing, she jumped into her father's bed, she lay down on her belly and kicked with her legs, and called at the same time, "*Look! does papa do*

¹ Franz Riklin.

that?" The analogy to the horse of "little John" which raised such disturbance with its legs, is very surprising.

With this last performance the solving of the problem seemed to rest entirely, at least the parents found no opportunity to make any pertinent observations. That the problem should come to a standstill just here is not at all surprising, for this is really its most difficult part. Moreover we know from experience that not very many children go beyond these limits during the period of childhood. The problem is almost too difficult for the childish reason, which still lacks much irremissible knowledge without which the problem cannot be solved.

This standstill lasted about five months during which no phobias or other signs of complex elaboration appeared. After the lapse of this time there appeared premonitory signs of some new incidents. Anna's family lived at that time in the country near a lake where the mother and children could bathe. As Anna feared to wade farther into the water than kneedeep, her father once put her into the water, which led to an outburst of crying. In the evening while going to bed Anna asked her mother, "Do you not believe that father wanted to drown me?" A few days later there was another outburst of crying. She continued to stand in the gardener's way until he finally placed her in a newly dug hole. Anna cried bitterly and afterwards maintained that the gardener wished to bury her. To finish up with, Anna awoke during the night with fearful crying. Her mother went to her in the adjoining room and quieted her. Anna dreamed that "a train passed and then fell in a heap."

We have here repeated the "stage coach" of "little John." These incidents showed clearly enough that there was again fear in the air, *i. e.*, that there again had arisen a resistance against the transposition on the parents, and that therefore a larger part of the love was converted into fear. This time suspicion was directed not against the mother, but against the father, who she was sure must know the secret, but would never let anything out. What could the father be secreting or doing? To the child this secret appeared as something dangerous, so that she felt the worst might be expected from the father. (This feeling of childish anxiety with the father as object we see again most distinctly in adults, especially in dementia præcox, which lifts the veil of obscurity from many unconscious processes, as though it were following psychoanalytic principles.) It was for this reason that Anna apparently came to the very absurd conclusion that her father wanted to drown her. At the same time her fear contained the thought that the *object of the father had some relation to a dangerous*

action. This stream of thought is no arbitrary interpretation. Anna meanwhile grew up a little and her interest for her father took on a special coloring which is hard to describe. Language possesses no words to describe the very special kind of affectionate curiosity which radiated from the child's eyes.

Anna once took marked delight in assisting the gardener while he was sowing grass, without apparently divining the profound significance of the child's play. About a fortnight later she began to observe with great pleasure the sprouting young grass. On one of these occasions she asked her mother the following question: "Tell me, how did the eyes grow into the head?" The mother told her that she did not know. Anna, however, continued to ask whether the Lord or her papa could tell this? The mother then referred her to the father, who might tell her how the eyes grew into the head. A few days later there was a family reunion at a tea, and after everything was over the guests departed. The father remained at the table reading the paper and Anna also remained. Suddenly approaching her father she said, "Tell me, how did the eyes grow into the head?" Father: "They did not grow into the head; they were there from the beginning and grew with the head."

A. "Were not the eyes planted?"

F. "No, they grew in the head like the nose."

A. "Did the mouth and the ears grow in the same way? and the hair, too?"

F. "Yes, they all grew in the same way."

A. "And the hair, too? But the mousies came into the world naked. Where was the hair before? Were there no seeds added?"

F. "No, you see, the hair really came out of little grains which are like seeds, but these were already in the skin long before and nobody sowed them." The father was now getting concerned; he knew whither the little one's thoughts were directed, but he did not wish to overthrow, for the sake of a former false application, the opportunely established seed-theory which she had most fortunately gathered from nature; but the child spoke with an unwonted seriousness which demanded consideration.

Anna (evidently disappointed, and with a distressed tone): "But how did Freddy get into mama? Who stuck him in? and who stuck you into your mama? Where did he come out from?"

From this sudden storm of questions the father chose the last for his first answer. "Just think, you know well enough that Freddy is a boy; boys become men and girls women. Only women and not men can have children; now just think, where could Freddy come out from?"

A. (Laughs joyfully and points to her genitals): "Did he come out here?"

Father: "Yes, of course, you certainly must have thought of this before?"

A. (Overlooking the question): "But how did Freddy get into mama? Did anybody plant him? Was the seed planted?"

This very precise question could no longer be evaded by the father. He explained to the child, who listened with the greatest attention, that the mother is like the soil and the father like the gardener; that the father provides the seed which grows in the mother, and thus gives origin to a baby. This answer gave extraordinary satisfaction; she immediately ran to her mother and said, "Papa has told me everything, now I know it all." She did not, however, tell what she knew.

The new knowledge was, however, put into play the following day. Anna went to her mother and said, "Think, mama, papa told me how Freddy was a little angel and was brought from heaven by a stork." The mother was naturally surprised and said, "No, you are mistaken, papa surely never told you such a thing!" whereupon the little one laughed and ran away.

This was apparently a mode of revenge. Her mother did not wish or was not able to tell her how the eyes grew into the head, hence she did not know how Freddy got into her. It was for this reason that she again tempted her with the old story.

I wish to impress firmly upon parents and educators this instructive example of child psychology. In the learned psychological discussions on the child's psyche we hear nothing about those parts which are so important for the health and naturalness of our children, nor do we hear more about the child's emotions and their conflicts; and yet they play a most important rôle.

It very often happens that children are erroneously treated as quite imprudent and irrational beings. Thus on indulgently remarking to an intelligent father, whose 4-year-old daughter masturbated excessively, that care should be exercised in the presence of the child which slept in the same room with the parents, I received the following indignant reply, "I can absolutely assure you that the child knows nothing about sexual matters." This would recall that distinguished old neurologist who wished to abjudicate the attribute "sexual" from a child-birth phantasy which was represented in a dreamy state.

On the other hand a child evincing a neurotic talent exaggerated by neurosis may be urged on by solicitous parents. How easy and tempting it would have been, *e. g.*, in the pres-

ent case, to admire, excite, and develop prematurely the child's eager desire for learning, and thereby develop an unnatural *blasé* state and a precociousness masking a neurosis. In such cases the parents must look after their own complexes and complex tendencies and not make capital out of them at the expense of the child. The idea should be dismissed once for all that children are held in bondage by, or that they are the toys of, their parents. They are characteristic and new beings. In the matter of enlightenment on things sexual it can be affirmed they suffer from the preconceived opinion that the truth is harmful. Many neurologists are of the opinion that even in grownups enlightenment on their own psychosexual processes is harmful and even immoral. Would not the same persons perhaps refuse to admit the existence of the genitals themselves?

One should not, however, go from this extreme of prudishness to the opposite one, namely that of enlightenment *à tout prix*, which may turn out as foolish as it is disagreeable. In this respect I believe the use of some discretion to be decidedly the wiser plan; still if children come upon any idea, they should be deceived no more than adults.

I hope, ladies and gentlemen, that I have shown you what complicated psychic processes the psychanalytic investigation reveals in the child, and how great is the significance of these processes for the mental well-being as well as for the general psychic development of the child. What I have been unable to show you is the universal validity of these observations. Unfortunately, I am not in a position to show this for I do not know myself how much of it is universally valid. Only the accumulation of such observations and a more far-reaching penetration into the problem thus broached will give us a complete insight into the laws of the psychic development. It is to be regretted that we are at present still far from this goal. But I confidently hope that educators and practical psychologists, whether physicians or deep-thinking parents, will not leave us too long unassisted in this immensely important and interesting field.

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ABSTRACTS OF LECTURES ON THE PSYCHOLOGY OF TESTIMONY AND ON THE STUDY OF INDIVIDUALITY¹

By PROFESSOR WILLIAM STERN (University of Breslau)

FIRST LECTURE

The Psychology of Testimony

1. *Introduction.* Applied Psychology in General. Along side of the purely theoretical psychology, which seeks a knowledge of the elements and laws of the mental life, there is now springing up, as an independent science, an "Applied Psychology." Its purpose is to gather such psychological information as will serve other sciences and especially the practical cultural activities of Education, Law and Medicine. In each of these fields Applied Psychology has a double task: As "Psychognostics" it must provide a scientific basis for practical knowledge of, and judgments upon, human mental acts and qualities; and as "Psychotechnology" it must give assistance in the practical manipulation of human minds.

An uncritical overestimation of this new science (psychologism) is as unreasonable as its underestimation.

A cardinal error, committed especially in the earlier days of this new science, was the attempt to carry over into it unchanged the methods of pure psychology; the thought was to apply the customary laboratory experiments (which, of intention, bring into artificial isolation the elementary psychical functions and are therefore remote from daily experiences) unaltered in the schools and in the courts, whereas the altered setting of the problem requires of course altered methods. Practical life does not deal with elements, but with very complex mental processes; the special methods of applied psychology must therefore take a middle position; they must combine the necessary nearness to life with that degree of exactness which is indispensable for the drawing of reliable inferences.

The Psychology of Testimony offers an illustration of these methodological points of view.

¹Lectures delivered at the Celebration of the Twentieth Anniversary of the opening of Clark University, Sept., 1909; abstracts prepared in German by Professor Stern, and translated by E. C. Sanford.

2. *The Methods of the Psychology of Testimony.* By "Testimony" or "Report" (*Aussage*) we mean the verbal expression of a recollection; and by "Recollection" (*Erinnerung*) a complex of memorial ideas which has reference to a definite objective constellation of facts (*Tatbestand*) in the past. The chief purpose of the study of testimony is the determination of its accuracy, *i. e.*, the degree of its agreement with the actual constellation of facts, and of the conditions upon which this accuracy depends. All experimental methods must therefore permit a comparison of the testimony with the facts to which it relates. Pictures to be carefully examined and then later described from memory furnish the most convenient material and have been most frequently used so far; but since in practical life one has very often to do with reports regarding events, event-experiments (*Vorgangsexperimente*) have also been arranged. Besides these, reports of narratives, of extents of time and space and many other matters have also been worked with.

The comparison of the facts and the report can be made only when the latter has been analyzed into its single statements (*Einzelangaben*) and the percentage of right, wrong and indeterminate statements has been calculated; and, because of the varied character of the statements, it is necessary to calculate separately the proportions for particular categories (*e. g.*, with reference to matters of color or of space relations) as well as to make a general calculation.

Next the conditions affecting the accuracy of the reports must be varied experimentally. The most important difference is here between the "narrative" (*Bericht*) and the "interrogatory" (*Verhör*, testimony given in response to questions, Whipple's "deposition"). In the case of the "interrogatory" form there are also different degrees of suggestion to be considered. Then the interval between the original observation and the report is to be varied; and finally the dependence of the accuracy of the report on the education, age and sex of the person under investigation is to be determined.

3. *Numerical Results.* The first experiments were made with pictures on mature students, both ladies and gentlemen. The reports were in the "narrative" form, without interrogation. The errors in the report, when made immediately after the observation, amounted to 5%, some weeks later to 10%. To distinguish those portions of the report with reference to which the subjects were very certain—so certain that they would be willing to take oath upon them—such portions were underscored. These portions showed a lessened tendency to error, but were not free from it.

(A further experiment, with the picture of the living room

in a peasant's house, was made upon children and young people of different ages; and has often been repeated since. The "narrative" resulted in 5-10% of errors; the "interrogatory" in 25-30%. The power of the "suggestive" question showed itself to be dependent in large measure on age—50% of errors in the case of 7-year-olds, 20% in that of 18-year-olds.

An event-experiment was made in my seminar as follows: My lecture was interrupted by the entrance of a gentleman who spoke with me and took a book from the book-case, the performance having been exactly studied beforehand in all its details. The members of the seminar gave but little attention to what was going on. A week later they were required to report upon what had taken place. Result: "narrative" 25% of errors; "interrogatory" 50% of errors.

4. *On the Psychology of the "Narrative" Errors.* These errors fall into four groups:

a. *Errors of Apprehension*, committed during the observation: Overlooking of elements present; misapprehension in consequence of expectation or habituation (*falsche Assimilation*); Sense illusions, Errors of estimation.

b. *Real Errors of Memory*, arising in the interval between the observation and the reporting or in the course of the report: Filling up gaps in recollection in accordance with habit; The use of retained verbal expressions in an altered sense; Gradual amplification of the idea—thus, with reference to two trees in a picture the statement in the first report was "two trees," a week later "a grove," a week later still "a forest."

c. *Errors of Phantasy*: Retouching of the recollection (*Ausschmückung*); Unintentional blending of the imagined with the experienced, or of the experiences of different times; In the case of children, often a quite harmless playing with the report, or invention (*Fabulieren*).

d. *Lack of Will*: Too great credulity with reference to the ideas which offer themselves; too little self-criticism in the case of uncertain recollections.

SECOND LECTURE

The Psychology of Testimony

5. *The Psychology of the Errors in Interrogatory Reports.* Beside the errors which have their sources within the reporting subject, there are others which have an outside source, in particular in the interrogation (*Verhör*). In one way questioning is an excellent means of filling the gaps in a spontaneous report, but in another it is, as experiments prove, a possible

occasion of falsification. The more dependent and easily influenced a man is, the more a question put to him operates as an imperative: You have to know something about this. And as he has usually exhausted in his narrative his store of clear and distinct ideas with reference to the experience, he hunts now among the remaining indistinct and fragmentary recollections for something wherewith to meet the question. This is true of all questions, but in an increased measure of *suggestive questions*, i. e., of those for which a particular answer is readier than others. For the question, "Was the cloth not red?" the answer "Yes" is always readier than the answer "No." The naïve human being is much inclined to affirm any idea presented to him, that is, to credit it with an objective existence. Suggestive questions of this sort operate with especial force in the case of young and uneducated persons; more with women than with men.

The suggestive question is only a special case of suggestion in general, the importance of which in normal psychology has only recently begun to be recognized. We define suggestion, from the standpoint of the person influenced, as "the imitative assumption of a mental attitude under the illusion of assuming it spontaneously" (*Nachahmung einer Stellungnahme unter dem Scheine des eigenen Stellungnehmens*).

Besides the influence of interrogation there are still others which falsify testimony: hearsay, reading about the occurrence, discussion with others who have shared the experience, etc.

6. *Practical Consequences for Pedagogy.* These are of a threefold sort:

a. In school and at home one has constantly brought before him reports by children as to experiences which they have had or stories which they have heard. It is clear that these reports are not worthy, off-hand, of full credence; the above mentioned sources of error must be reckoned with. It is clear also that a report demonstrably false is not necessarily to be regarded as a lie and punished accordingly. The unconscious factors of falsification play a far greater rôle than is commonly supposed; and if one condemns in the case of little children every mistake and every harmless tale of fancy as a lie, he usually succeeds in giving to the child in this way a conception of which the child would otherwise perhaps have remained in ignorance.

b. Since a large part of the falsification in the report is usually a result of questioning, the questioner is himself co-responsible for the false report of the child. These falsifications are for the most part unconscious; and yet they may, under certain circumstances, give place to conscious falsifications, since the child sometimes knows no other way of escaping

the disagreeable compulsion of the question than the invention of an answer. One should therefore interrogate no more than is absolutely necessary and should formulate his questions as "unsuggestively" as possible.

c. Since memory is such an important function it is natural to ask whether we must rest satisfied with its demonstrated imperfection. Is it not possible to secure an improvement by pedagogical means? The question is to be answered affirmatively; just as observation (*Anschauung*) can be systematically cultivated, so can memory. The improvement is subject to experimental demonstration; experiments repeated on the same children (each time of course with a new picture) showed a clear improvement. The chief educative effect was in this case due to *self-correction*. After the making of the report the picture was shown again to the child and he was required himself to discover the errors which he had made. Such exercises of memory may be scattered through the work upon any school topic as opportunity offers.

7. *Practical Consequences for Law.* (The consequences here mentioned have reference in the first instance to German jurisprudence and court procedure. To what extent analogous points of view hold also for American conditions those familiar with the latter must decide.)

a. The first and obvious consequence of the psychology of testimony is a negative one, a diminution of the reliance which is to be placed in the reports of witnesses. The notion, still tolerably prevalent, that the faithfully sworn testimony of a mentally competent witness is in general to be regarded as an exact presentation of reality, is without justification. In Germany the new view has already caused the testimony of children especially to be less highly valued than formerly.

It would be a mistake, however, to ascribe to the psychology of testimony destructive consequences only; its *positive consequences* are still more important.

b. The examining officer is able by the manner of his questioning to predetermine in a measure the degree of the erroneousness of the testimony. The more he leaves to spontaneous narration, and the less "suggestive" his questions, the less will be the danger of falsification.

c. When identification is necessary the witness should make it, whenever possible, by choice from a group of similar persons or things (*Wahlkonfrontation*) and not by indicating whether a single individual presented to him is the one in question (*Einzelkonfrontation*), because of the powerful "suggestive" effect of the latter procedure.

d. Psychological experiment shows what degree of confidence ought in general to be placed in particular classes of

testimony. It teaches, for example, that colors to which particular attention has not been given are especially ill remembered; that times of a few minutes are almost always considerably over-estimated; that the main outlines of an event, if they have been followed with attention and if the witness has not shared especially in the emotions involved, are commonly correctly reproduced; that on the contrary, things observed without attention are very liable to distortion. (For this reason delayed reports with reference to the appearance or clothing of a person not carefully observed are for the most part worthless.)

e. As the psychological study of testimony advances it will become possible for experts of psychological training in *exceptional cases* to offer opinions on the psychical character of important witnesses; the experimental testing of witnesses also, *e. g.*, with reference to their capacity to observe, their suggestibility, their ability to estimate extents of time and space, their memory for colors, seems at least in principle, possible; though, so far, psychological methods are not ripe for it.

f. The best thing of all is, of course, that the jurist himself should be a psychological expert; for this reason it is before all else desirable that jurists should be thoroughly trained in applied psychology and its methods and results. A jurist who has himself been the subject of experiment and thus has seen in his own case how memory functions and how the answering of questions (*Verhörsfragen*) is actually performed, as well as on what conditions these operations depend, will profit from the experience in the technique of his own questioning.

g. The testimony of adolescents and children demands special consideration. While the juvenile offender before the court receives a wholly different treatment from the adult, the juvenile witness is not thus distinguished. It is not borne in mind that the usual procedure of interrogation greatly diminishes the value of child testimony and at the same time puts the juvenile witness in moral peril. The introduction of special investigating magistrates (*Untersuchungsrichter*) for juvenile witnesses, before whom the children should be examined but once and then as soon as possible after the event, is to be desired.

8. *Literature.* The most important titles in the literature of the Psychology of Testimony, as well as collective reviews of it, are to be found in W. Stern's *Beiträge zur Psychologie der Aussage*, 2 vols., Leipzig, 1903-6, and in its continuation, the *Zeitschrift für angewandte Psychologie*, edited by Stern and Lipmann since 1907. In America G. M. Whipple gives in the *Psychological Bulletin*, VI, No. 5, May, 1909, a collective review with bibliography; and a very extended bibliography has also been brought together by Wigmore in the *Illinois Law Review*, III, Feb., 1909.

THIRD LECTURE

The Study of Individuality: General, Psychography

1. *Problems.* In addition to the main problem of Psychology (the investigation of the general uniformities of the mental life) two others now begin to engage attention, which until recently, have been left almost wholly to other disciplines.

The Question of Differences (differentielle Fragestellung) deals with the variations in the particular mental functions. Each may be studied with reference to the degree of its general variability; its qualitative differentiation into "Types," its quantitative differentiation into grades, its genetic differentiation into developmental stages, its relative variations in comparison with other functions, *i. e.*, its correlation.

The Question of Individuality (individuelle Fragestellung) has to do with the knowledge of a single individual personality in and for itself, whether in relation to its total psychical make-up or in relation to a particular aspect, as character, intelligence, etc.

The study of individuality has, up to the present, been a matter either of the historical sciences (biography) or of certain practical disciplines (lists of individualities in schools, alienists' tests of intelligence, characterological indications of the graphologists and the like). There is needed, however, both from a philosophical and from an empirico-methodological point of view, a general scientific foundation for all these undertakings.

2. *The Philosophical Basis of the Concept of Individuality* can here be merely indicated,—for details confer W. Stern's *Person und Sache, System der philosophischen Weltanschauung*, I, and his *Psychologie der Individualität*, to appear in 1910. It is impossible to take an individuality merely as an aggregate of contents of consciousness; for, on the one hand, that which appears in consciousness is by no means identical with the real and essential kernel of individuality; and on the other, the multiplicity of the psychical content is combined into a single organic whole which can be explained only by a unitary purposeful principle of activity (*aus einem einheitlichen zielstrebigem Tätigkeitsprinzip*). Every individuality is therefore a "person" in the sense of the following definition: "A person is such an existence as, in spite of the multiplicity of its parts, presents a real unity, having a character and a value of its own; and as such exhibits, in spite of a multiplicity of subordinate functions, a unitary and purposeful self-activity." (*Person ist ein solches Existierendes, das trotz der Vielheit der Teile eine reale eigenartige, eigenwertige Einheit darstellt, und als solche trotz der Vielheit der Teilfunctionen eine einheitliche, zielstrebige, Selbsttätigkeit vollzieht.*)

The source of the individual character is to be found neither alone in what is innate (Nativism), nor alone in the operation of outer conditions (Empiricism); but on the contrary, every single phenomenon arises through "convergence of outer and inner factors" (the Convergence Theory).

Since, therefore, the inner capacity of a personality is but a single conditioning factor, which must be supplemented by others, it may properly be called a "Disposition." The disposition of an individual shows itself in a series of single tendencies and capacities which fall into two chief groups; that of the *innate tendencies* (*Anlagen*, developmental tendencies), and that of the *characteristics* (*Eigenschaften*, *Beharrungstendenzen*).

3. *Methodological.* New methods for the empirico-psychological investigation of individuality are now being worked out at different places in Europe. The correlation of psychical characteristics is being studied by Spearman and Kruger and by Heymans; Heymans, Sommer and others are engaged upon inheritance of psychic characteristics, the study of families and the like; in England the recently founded Eugenics Laboratory is especially devoted to this problem. With the problem of the mental endowment and intelligence of school children are busy Binet, Meumann, Stern and others. "Pathography," the analysis of distinguished personalities from a psychopathological point of view, has been developed by Möbius and his followers. "Psychography," as the common foundation of all the methods of individual psychology, is being worked out in our *Institut für angewandte Psychologie*.

4. *Problem and Tendency of Psychography.* All studies of individuality so far suffer from one common defect: The selection of the characteristics and attitudes which have been tested in the individuals examined has been a matter of chance, dependent on subjective preference or preconceived meaning on the part of the investigator. Each biographer has reported particular marks as "essential" for the characterization of his hero; the rest he has disregarded or merely touched upon. No two alienists have made use of the same experiments for testing the intelligence of their patients; arbitrarily selected individual functions have been regarded as symptomatic. Of like arbitrary selection are all the lists of "mental tests" so far proposed; and accident has in the same way determined the rubrics of the "individuality books" and the lists that have been made use of in many schools. The collection of such lists, questionnaires and formularies of individuality, brought together in the Berlin *Institut für angewandte Psychologie*, shows that a veritable chaos reigns in this matter and that in

consequence a comparison of the tests of individuality made use of by different observers is nearly impossible.

It has seemed to us, therefore, a precondition of all further work in this field that a "Scheme of Psychography," of as complete a kind as possible should be worked out, *i. e.*, "a list, arranged in a synoptical manner, of all those characteristics which can in any possible way come into consideration in the study of individuality, without reference to *à priori* assumptions as to whether or not they are essential or to the special purposes of particular studies of character."

The Scheme has not been brought to a point at which each student of individuality may simply fill it out for the personality which he is studying; it furnishes, rather, the stock from which he may select the procedure appropriate to his object. But he must now give account to himself as to why he chooses just the particular points which he does and omits others; and he will take into consideration many points of which he would not otherwise have thought. The Scheme will further be indispensable in all genuine psychological investigations of individuality, correlation, inheritance and the like.

The Scheme must, of course, be completely *neutral*, *i. e.*, it must include the points of view of the historian, the alienist and the educationist as well as that of the psychologist; it must also make specific, for the study of supernormal endowment, the point of view of artistic creation, of scientific production, etc. It is easy to see that such an undertaking can only be carried through by the co-operation of many workers of many professions. The *Institut für angewandte Psychologie* has therefore formed a Commission for Psychography by which recently, after many years of work, a beginning of publication has been made. ("*Ueber Aufgabe und Anlage der Psychographie*" and "*Fragment eines Schemas der Psychographie*" in the *Zeitschrift für angewandte Psychologie*, III, Heft. 3.)

The trend of the Scheme is as follows: When an individual is to be "psychographed," a sharp distinction must be made between the "attitudes" (*Verhaltensweisen*) to be observed directly and the "characteristics" (*Eigenschaften*) to be inferred from them. The catalogue of attitudes falls again into two groups, according as we have to do with "natural" attitudes or those under *experimental* conditions. (Just these natural attitudes resist all schematization as yet; nevertheless they are, on the one hand, the chief material of biographical-historical studies, and, on the other, in the case of psychographing a living individual who can be subjected to experiment, indispensable for completeness. In view of this, the Scheme must try to formulate more exactly the data with reference to natural attitudes in such a manner that they may appear as reactions to

definite stimuli occurring in the course of life, *e. g.*, the attitude toward money, attitude toward affairs, attitude toward extraordinary occurrences.) Finally the Scheme must possess in the greatest possible fullness rubrics covering the *Ætiology* and *Symptomatology* of the individual to be psychographed. *Ætiology*: Data with reference to inheritance, diseases, character of the family, influences of nurture and education, etc. *Symptomatology*: Data with reference to physical form, body mass, physiognomy, expressive movements, voice, etc.

At the start the Scheme will of course contain many *lacunæ* which can be discovered and filled only as it is actually put to use; it is therefore desirable that for the immediate present the Scheme should be put to the test of varied application—historical, psycho-pathological, pedagogical, psychological.

FOURTH LECTURE

The Study of Individuality: The Individuality of the Child

1. *The Little Child.* The conditions for the thorough study of individuality are most favorable in the case of the child during his first six years; for then uninterrupted observation is possible for the parents; then the outer influences can be followed without break, and the empirical and nativistic elements of development can be clearly separated; the expressions of the mental life are still relatively simple; and the children are unconstrained before the observer. In these studies of little children a change from the method so far pursued is desirable, and in this direction, namely: parents must give themselves more and more to co-operative work in observation. The usual limitation to the first three years of life should be given up; many functions begin to show their most interesting development only between the fourth and sixth years. Observation, moreover, should not be confined alone to the most elementary functions; the development of feeling and of character, play, drawings, thought, children's views of the world, and many other matters must be described just as minutely. The various observers must work more from common points of view in order that their results may be more readily comparable. (With this object in view the *Institut für angewandte Psychologie* is beginning to issue a series of guides to the observation of child development.)

2. *The Child of School Age.* Here can be mentioned but one of the most important problems, which requires the close co-operation of Pedagogy and Psychology: *In what way should the organization of the schools and classes be adapted to the differences in the individuality of the children?* So far the organization of the schools has been undertaken almost exclusively

from the objective points of view of the differences in social station and of future calling. Along with these a psychological method of regarding the matter is now beginning to have influence.

3. *The Differences of the Sexes.* The segregation of boys and girls in separate schools has always been undertaken chiefly on non-psychological grounds; and the supporters of co-education have likewise on their part been persuaded that fundamental psychic differences do not exist. Certain psychological experiments seem to confirm this; but these again have been made upon elementary functions, and in these the true differences do not come to light. Studies dealing with complex and higher forms of activity (the relation of receptivity to spontaneity, the direction of interests, spontaneous drawings) show unquestionable differences, which cannot be attributed to influences of the outer *milieu*, but must be regarded as innate. The rhythm and tempo of development also are different in the two sexes.

As co-education is practiced in America to a greater extent than elsewhere, the opportunity for a purely psychological study of the problem is especially favorable.

4. *Organization with Reference to Grade of Endowment.* Differences in the intelligence of children were first recognized in school organization when schools for backward children (*Hilfsschulen*) were introduced. The great mass of "normal" children, however, remained still undifferentiated, though they show extremely marked differences in endowment. The fact that there is "repeating" (the necessary repetition of a grade's work) shows clearly that a certain percentage of the pupils always falls behind the requirements of the class. This circumstance caused Schulrat Sickinger of Mannheim to arrange special classes for these less well endowed pupils. These classes (called *Sonderklassen* or *Forderklassen*) have a less amount to do, fewer pupils per class and a different course of study. They have already been initiated in the larger cities of Germany and seem to justify themselves.

5. *Binet's Tests for Establishing a Scale of Intelligence.* The practical efforts to classify children according to grade of endowment just mentioned demand, however, that reliance should not be placed on the unsupported judgment of the teacher, but that more exact means of determining the capacities of the children should be secured. Many efforts have been made to establish "tests," but all so far have gone to pieces, as far as their main purpose was concerned, for the following reasons: Too much was attempted; it was thought that one might secure by a short series of experimental probings, in a very brief time, a total picture of the individuality.

The list was limited (especially in the older series of tests) far too much to the elemental functions of sense perception, reaction times, mechanical memory, etc., though just these functions are far less characteristic of the special features of individuality than the complex functions. In the case of the complex functions again, it is very hard to separate the actual *capacities* of intelligence (*Intelligenzanlagen*), which it is desired to test, from the objective effects of instruction, training, etc. Thus, for example, many of the alienists' so-called tests of intelligence are really tests of information and scholastic attainment.

It seems, however, that Binet, who has, for more than a decade, been making unwearied studies of the intelligence of school children (cf. the general review by Bobertag, *Zeitschrift für angewandte Psychologie*, III, Heft. 3), has now at last hit upon a practicable method. Its chief advantage consists in this that Binet has determined empirically which of his tests correspond normally to the ability of children at different ages. He is thus in a position to grade each child according to his intelligence-age (*Intelligenzalter*) and thus to say whether his actual age corresponds to this intelligence-age, or whether the child is ahead in his development by one or two years, or behind. It is even possible in this way to assign to the adult feeble-minded their mental level by comparison with the corresponding age of children. Binet has tried to arrange his tests in such a way that they are as independent as possible of acquired knowledge. While he has not attained perfect success, it has been possible, nevertheless, to apply his method with few changes to German children, though in their case the external conditions of instruction are in some respects wholly different. It would seem, therefore, that we are here on the way to a generally applicable method of testing grades of intelligence.

6. *Supernormal Endowments.* Children who differ from normality on the side of excess have so far received the very least consideration from a psychological and pedagogical point of view. Though the supernormal are relatively few in number, their significance for society and human progress is very great. Many "infant prodigies" (*Wunderkinder*) come to early injury in mind and body because they are thoughtlessly forced into publicity. In the schools, on the other hand, exceptional talents may remain undiscovered, because they cannot reveal themselves in the school machine, which must be adapted to the average; and thus they run the risk of degeneration. Kerschensteiner has discovered such instances in the case of artistic endowment. Finally the schools are dangerous for those of exceptional powers because such children do not have

to bring their powers to maximal tension; they do not have to steel their wills and train their sense of duty.

In this case, as before, preliminary pedagogical and psychological work must be done. Supernormal young people must be psychographed with the greatest precision and with reference to every sort of endowment. The significance of inheritance, *milieu*, and education in their development must be determined. We must discover whether there is not possible, even in early life, a clear differentiation between "infant prodigies" in the strict sense (*i. e.*, children with accelerated development, who later soon come to a standstill) and real child geniuses, like Mozart, who afterward as adults retain their supernormality. Finally, the correlation between different sorts of supernormal endowments must be studied, and that between supernormal endowment and general intelligence.

From the practical point of view there must be roused in society the conviction that it has special duties not only toward the mentally inferior, but also toward the mentally superior. Proposals have already been made with this in view which must be tested: Separate classes in the common schools for specially brilliant pupils; élite gymnasia, with wholly different courses of study and different requirements, for picked groups of those most exceptionally talented, without reference to family or property; legal measures to protect the "infant prodigies" from exploitation and to secure for them, remote from publicity, suitable training in their specialties combined with general education.

FREUD'S THEORY OF DREAMS¹

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Freud's theory of dreams occupies a nodal position in his psychology, constituting as it does a point of conjunction for his various conclusions on normal and abnormal mental life. From it as a starting-point he has developed outlooks that call for the earnest consideration of psychologists, for it is extensively conceded that if his conclusions are true they carry with them a revolutionary change in our knowledge of the structure and functions of the mind. These broader aspects of his theory will not here be considered,² the present paper being intended merely to delineate the main outlines of the dream theory proper. Owing to the richness of the subject-matter even this purpose can here of necessity be but very imperfectly attained, so that the following description can at best only serve as an introduction to the study of the *Traumdeutung*.³ No just criticism of the theory can be made without a careful perusal of this volume, in which Freud has in detail entered into all the manifold problems relating to dreams, has presented the evidence on which his conclusions are based, and has fully discussed rival views and anticipated the possible objections that may be raised to his own. A few illustrative examples, drawn from the writer's experience, will accompany the present paper, but in order to economise space no dream-analyses will be detailed, it being proposed to do this in a subsequent article.

The method Freud uses in the investigation of dreams is that termed by him Psycho-Analysis, and on the question of the reliability of this method rests that of the validity of his conclusions. No account of psycho-analysis itself can be given here,⁴ for that alone would exact a long exposition, but

¹ Amplified from a paper read before the American Psychological Association, Dec. 29, 1909.

² A general sketch of Freud's psychology is given in the April number of the *Psychological Bulletin*.

³ 1st ed. 1900, 2d ed. 1909.

⁴ Without considering any questions of technique I have elsewhere given brief accounts of it (*Journal of Abnormal Psychology*, June-July, 1909, and *Journal of Nervous and Mental Disease*, 1910). Freud's account of the original technique, which has later been considerably elaborated and modified, may be found in Dr. A. A. Brill's translation, entitled *Selected Papers on Hysteria (Journal of Nervous and Mental Disease, Monograph Series, No. 4, 1909)*.

it should explicitly be stated that the technique of this method is a complex and intricate matter, the acquirement of which is not, as many writers seem over-readily to assume, an easy task, but one requiring much practice, patience and experience. In no branch of science can the testing of the results obtained by the use of an entirely new and difficult technique be satisfactorily submitted to an off-hand trial on the part of some one quite untrained in this, and it is strange that it does not occur to those who do not directly confirm Freud's conclusions as soon as they "try psycho-analysis" that the fact may be due, not, as they hastily infer, to the erroneusness of those conclusions, but to a more humble explanation, namely that they have not mastered the technique. It is at all events striking that up to the present no investigator, in any country, who has taken the trouble to learn the technique of the psycho-analytic method, has reached any conclusions that fail to confirm Freud's in all particulars, although at least fifty thousand dreams have been investigated by this method; this fact in itself speaks for the finished state in which Freud gave the theory to the world.

It is commonly believed in scientific circles that the mental processes of which dreams are composed arise, without any direct psychical antecedent, as the result of irregular excitation of various elements in the cerebral cortex by physiological processes occurring during sleep. This, it is maintained, accounts for the confused and bizarre nature of the mental product, and any apparently logical connection and order that frequently appear to some extent in dreams are explained by the supposition that the mental processes in question are represented in cortical elements that stand in close, anatomical or physiological, relation to one another, and so are simultaneously stimulated by the peripheral stimuli. Hence any problem as to the psychical origin of the mental processes, still more as to the *meaning* of the dream as a whole, is by the nature of things excluded as being non-existent, and any investigation along such lines is condemned as savouring of antiquated superstitions about the "reading of dreams" unworthy of educated people. To this attitude Freud, as must every consistent philosopher, stands in sharp opposition. He contends that dream processes, like all other mental processes, have their psychical history, that in spite of their peculiar attributes they have a legitimate and comprehensible place in the sequence of mental life, and that their origins can be psychologically traced with as much certainty and precision as those of any other mental processes.

From one point of view dreams may be classified into the following three categories. First may be distinguished those

that are at the same time sensible and intelligible; such especially are the dreams of children. The very occurrence of such dreams, in which the mental processes fully resemble those of waking life, although they are never confounded with them, is in itself a strong argument against the view that dreams result from the isolated activity of single groups of brain cells. Secondly, there are dreams which are connected and have an evident meaning, but one the content of which is curious and surprising, so that we cannot fit them into the rest of our waking life. A person dreams, for instance, that his brother has been gored to death by a bull; he cannot account for his having come by such a curious notion nor can he relate it to any waking thought. Thirdly, there is the most frequent type of dream, where the mental processes seem disconnected, confused and senseless. These two latter types of dreams have a peculiar quality of strangeness and unreality; they are foreign to the other mental experiences of the subject, and cannot be inserted into any place in his waking thoughts. It is as though the subject has lived through a different range of experience, in another place or in another world, which apparently has no connection with the one to which he is accustomed. Now Freud holds that this sense of foreignness is an illusion, due to very definite causes, and that the mental processes that go to form dreams are in direct continuity with those of waking life.

In tracing the antecedents of dream processes Freud makes use, as has been said, of the psycho-analytic method, which essentially consists in the collecting and ordering of the *free* associations that occur to the subject when he attends to any given theme and abrogates the selecting control over the incoming thoughts that is instinctively exercised by the conscious mind. If this method is applied to any component part of a dream, however senseless it may appear on the surface, mental processes are reached which are of high personal significance to the subject. The mental processes thus reached Freud terms the "dream thoughts" they constitute the "latent content" of the dream in contradistinction to the "manifest content," which is the dream as related by the subject. It is essential to keep distinct these two groups of mental processes, for on the appreciation of the difference between them rests the whole explanation of the puzzling riddles of dreams. The latent content, or dream thought, is a logical and integral part of the subject's mental life, and contains none of the incongruous absurdities and other peculiar features that characterise the manifest content of most dreams. This manifest content is to be regarded as an allegorical expression of the underlying dream thoughts, or latent content. The distor-

tion of the dream thoughts into the dream proper takes place according to certain well-determined psychological laws, and for very precise reasons. The core of Freud's theory, and the most original part of his contribution to the subject, resides in his tracing the cause of this distortion to a "censor" which interposes an obstruction to the becoming conscious of unconscious psychical processes. This conception he arrived at from the analysis of various abnormal psychical manifestations, psycho-neurotic symptoms, which he found to be constructed on a plan fully analogous to that of dreams. It may be remarked at this point that, quite apart from any views as to the cause of the distortion, the nature and functions of the dream thoughts and other problems, the fact itself of the distortion is certain, and cannot be doubted by any one who carefully observes a few dreams. That, for instance, the vision of a strange room in a dream is a distorted presentation of several rooms that have been actually seen, from each of which various individual features have been abstracted and fused together so as to present a new and therefore strange room, is the kind of observation that can easily be verified. Before considering, therefore, the nature of the latent content it will be well shortly to describe the distorting mechanisms by means of which it becomes transformed into the manifest content.

A dream is not, as it appears to be, a confused and haphazard congeries of mental phenomena, but a distorted and disguised expression of highly significant psychical processes that have a very evident meaning, although in order to appreciate this meaning it is first necessary to translate the manifest content of the dream into its latent content, just as a hieroglyphic script yields its meaning only after it has been interpreted. The mechanisms by means of which the manifest content has been formed from the underlying dream thoughts may be grouped under four headings.

The first of these is called *Condensation* (*Verdichtung*). Every element of the manifest content represents several dream thoughts; it is, as Freud puts it, "over-determined" (*überdeterminiert*). Thus the material obtained by analysis of a dream is far richer and more extensive than the manifest content, and may exceed this in amount by ten or twenty times. Of all the mechanisms it is the easiest to observe, and to it is mainly due the sense of foreignness that dreams give us, for it is a process with which our waking thought is not familiar. The representation in the manifest content of the extensive material comprising the latent content is brought about by a true condensation, rarely by the mere omission of part of the latent content. The condensation is effected in several ways. A figure in a dream may be constituted by the

fusion of traits belonging to more than one actual person, and is then called a "collective person" (*Sammelperson*). This may occur either by the fusion of some traits belonging to one person with some belonging to another, or by making prominent the traits common to the two and neglecting those not common to them; the latter process produces a result analogous to a Galton's composite photograph. The same process frequently occurs with names: thus Freud mentions a dream in which the person seemed to be called Norekdal, which had been formed from the names of two of Ibsen's characters, Nora and Ekdal; I have seen the name Magna formed by fusing Maggie and Edna, and similar instances are common enough. The neologism thus produced closely resembles those met with in the psychoses, particularly in dementia præcox, and like these may refer to things as well as to persons. Lastly in this connection it should be remarked that certain of the elements in the manifest content are especially rich in associations, as if they formed particular points of junction (*Knotenpunkte*); they are in other words the "best-determined" elements. These are intimately related to the most significant elements in the underlying dream thoughts, and frequently show the greatest sensorial vividness in the manifest content.

Condensation subserves more than one function. *In the first place* it is the mechanism by means of which similarity, agreement or identity between two elements in the latent content is expressed in the manifest content; the two elements simply become fused into one, thus forming a new unity. If this fusion has already taken place in the latent content the process is termed *Identification*, if it takes place during the construction of the dream itself the process is termed *Composition* (*Mischbildung*); the former process rarely concerns things, chiefly persons and places. In the process of identification a person in the dream enters into situations that really are proper to some other person, or behaves in a way characteristic of this second person. In the process of composition the fusion is revealed in the manifest content in other ways; thus a given person may appear in the dream, but bearing the name of some second one, or the figure in the dream may be composed of traits taken some from the first, others from the second person. The existence of a resemblance between two persons or places may thus be expressed in the dream by the appearance of a composite person or place built up in the way just mentioned; the important feature that the two have in common, which in this case is the essential constituent in the latent content, need not be present in the manifest content, and indeed usually is not. It is clear that by this means a con-

siderable economy in presentation is effected, for a highly complex and abstract resemblance may be expressed by simply fusing the figures of the persons concerned. Thus, if two persons both show the sentiments of envy, fear and malice towards the subject of the dream these sentiments may be expressed by the appearance in the manifest content of a composite figure of the two persons. In this composite figure there may be traits common to both persons, such as colour of hair or other personal characteristics, but the essential resemblance, which is the cardinal point in the underlying dream thoughts, is as a rule not evident in the dream. The superficial resemblance presented in the dream is frequently thus the cover for a deeper and more significant one, and gives the clue to important constituents of the dream thoughts. The process in question may also represent merely the wish that there were such a resemblance between the two persons, and therefore the wish that they might be exchanged in their relation to the subject. When, for instance, a married lady dreams that she is breakfasting alone with some man friend, the interpretation is often a simple matter. *In the second place* condensation, like the other distorting mechanisms, subserves the function of evading the censor of consciousness. This is a matter that will presently be further discussed, but it is plain that a repressed and unacceptable wish that two persons or places may resemble each other in an important respect, or may be interchanged, can be expressed in the manifest content of a dream by presenting an insignificant resemblance between the two.

It might be assumed from the description given above that the process of condensation takes place in one direction only, that each element in the manifest content represents a number of elements in the latent content in the same way that a delegate represents the members of his constituency. This, however, is not so, for not only is every element in the manifest content connected with several in the latent content, but every element in the latter is connected with several in the former. In addition to this, frequently associations exist between the different elements of the entire structure of the dream, so that this often has the appearance of a tangled network until the full analysis brings law and order out of the whole.

The second distorting mechanism is that termed *Displacement* (*Verschiebung*). In most dreams it is found after analysis that there is no correspondence between the psychical intensity of a given element in the manifest content and the associated elements in the latent content. An element that stands in the foreground of interest in the former, and seems to be the central feature of the dream, may represent the least

significant of the underlying dream thoughts; conversely an apparently unessential and transitory feature in the dream may represent the very core of the dream thoughts. Further, the most prominent affect in the dream, hate, anxiety and so on, as the case may be, often accompanies elements that represent the least important part of the dream thoughts, whereas the dream thoughts that are powerfully invested with this affect may be represented in the manifest content of the dream by elements of feeble affective tone. This disturbing displacement Freud describes, using Nietzsche's phrase, as a "transvaluation of all values." It is a phenomenon peculiarly frequent in the psycho-neuroses, in which a lively interest or an intense affect may be found associated with an unimportant idea. In both cases a transposition of affect has taken place whereby a highly significant idea is replaced by a previously indifferent and unimportant one. Often the association between the primary and secondary ideas is a very superficial one, and especially common forms of this are witty plays on the speech expression for the two ideas, and other kinds of clang association. As is well known, Jung has demonstrated¹ that this superficial association is usually the cover for a deeper hidden bond of high affective value. This mechanism of displacement is the cause of the puzzling fact that most dreams contain so many indifferent and hardly noticed impressions of the previous day; these, having on account of their unimportance formed but few associations with previous mental processes, are made use of in the dream-making to represent more significant ideas, the affect of which is transferred to them. Displacement also explains much of the bizarreness of dreams, notably the remarkable incongruity between the intensity of the affect and the intellectual content; a person may in a dream be terrified at an apparently indifferent object and quite at ease in the presence of what should be alarming danger.

Condensation and Displacement are the two main mechanisms by means of which is produced the distortion during the passage from the latent to the manifest content. The extent to which a given dream appears confused, bizarre and meaningless varies exactly with the extent to which these two mechanisms have been operative in its formation. The following fragmentary extracts from some dream analyses will illustrate the processes in question.

(1) *I recently dreamt that I was travelling in Italy on my way to the next Freudian Congress (which is to be held in March). On looking at my railway ticket I found it was for Lugaro. In reality I know of no place of that name, but I*

¹ *Diagnostische Assoziationsstudien.* Bd. I, 1906.

have pleasant memories of the charming Italian resort Lugano. In the dream I had replaced the *n* of this by the letter *r*. Now Nuremberg is the meeting-place for the Congress. Lugaro is the name of a well-known Italian psychiatrist. I am to my regret prevented from going to the Congress by having to give a psychiatry course at the time this is to be held. I have often described the neighborhood of Lugano as *toy* scenery; on my way to Nuremberg (the *toy* centre of the world) I am arrested (Luga-*no*) by the obstacle of my psychiatry course, and replace my destination by a name indicative of that fact. Further than this, I had recently seen the translation of Lugaro's "Modern Problems in Psychiatry," made by Orr and Rows; emphasis is thus cast on the last syllable of Lugaro's name by the play on the sound of "or" and "ro." The volume is chiefly concerned with problems of chemistry and morbid anatomy, and advocates a tendency in psychiatry the relative fruitlessness of which I have disparagingly contrasted with that pursued by Freud and Jung, both in the medical press¹ and, amongst others, in a conversation I had a couple of years ago (about Lugaro) with Dr. Rows. I deplore the translation of the book into English, for it will only serve to strengthen the materialistic trends, useful enough in their proper place, that already too exclusively occupy the thoughts of English psychiatrists. For reasons not under my control, my psychiatry course is likewise chiefly concerned with matters of chemistry and morbid anatomy, so that the enforced displacement of my Freudian interests by the tendency represented by Lugaro also finds expression in the negation of Nuremberg (Luga-*no*), and the replacement of it by a word indicating in detail the nature of the interruption. I might further add that Lugaro is professor at *Modena*, and that a friend of mine, Dr. *Modena* of Ancona, is the first Italian psychiatrist to accept Freud's views, as I was the first English one. I had just heard that Dr. *Modena* is engaged in translating Freud's works into Italian, in a sense a counterbalance to the translation of Lugaro's "Modern Problems" into English; even the other consonant of Modena's name, *n*, is concerned in the dream-making by its being displaced by the prominent consonant, *r*, of those of the two English translators, Orr and Rows (Lugaro instead of Lugano).

Associated, therefore, with only one word in the manifest content of the dream, which at first sight appeared to be meaningless enough, are a number of mental processes that occupy a significant place in my waking life. These, and many others which for personal reasons I cannot mention, are connected

¹ *Lancet*: July 24, 1909.

with the element in the manifest content of the dream by means of exceedingly superficial associations, chiefly ridiculous plays on words of a kind I hope I should never be guilty of when awake. Any one, however, who is interested in the psychology of wit, or familiar with the unconscious phantasies of hysterics or the flight of ideas met with in mania and other psychoses, will not find it strange that the superficial associations and preposterous plays on words so characteristic of those fields of mental activity are common enough in yet another field, namely that of dream formation. The question as to whether the associations that occur during dream analysis are made only then, and take no share in the actual formation of the dream, will not here be discussed; it is one of the objections with which Freud fully deals in the *Traumdeutung*.

Like the other ones to be quoted, this fragment is only a small part of the full dream, and I might add that the associations here related are only intermediate connections to more remote thoughts, which as the analysis deepened soon left the subject of psychiatry for a more personal one.

(2) *I was in the country in Massachusetts, and yet seemed to be in the east not of America but of England. Above a group of people was vaguely outlined the word Ölve or Ölde (which may be expressed as Ölæ).* This dream affords a particularly striking illustration of displacement, for every element in it directly led in the analysis to thoughts about the Netherlands, although no indication whatever of this country appeared in the manifest content. Massachusetts brought to my mind its capital Boston, and the original Boston in Lincolnshire¹. That reminded me of Essex,² these two counties being the most low-lying (Netherlandish) ones in England. In Essex lives a friend through whom I had got to know well a number of Flemish people. On the day preceding the dream I had written a letter to some one in Malden, a town in Essex, a name the sound of which brought to my mind Moll of Flanders. The costume of the people in the dream was taken from a picture of Rembrandt's, which brought up a number of recent and old memories. Ölæ was a condensation of Alva, the tyrant of the Netherlands, and Van der Velde, the name of a Flemish

¹That in the dream-making I was presumptuous enough to confound an American State with an English County is an illustration of the irresponsible liberties taken by the mental processes concerned in this production, and shows how completely they differ from our waking thoughts.

²I might add that the latter part of the word Massachusetts has a sound not very dissimilar to that of Essex, further that the signification of the first part of it, *chu* (chew, which in Boston is pronounced as if it were spelt *chu*) resembles that of the other word (*ess* is the stem of the German verb "to eat").

painter whose work (oil paintings, Öl is German for oil) has struck me in the Louvre, and also of a particular Flemish friend: two days previously I had seen in the hospital a Dutchman with a very similar name. In short, turn which way I would, all parts of the dream stubbornly refused to associate themselves with anything but Netherland topics, the analysis of which resolutely led in only one direction.

(3) A patient, a woman of thirty-seven, dreamt that *she was sitting in a grand stand as though to watch some spectacle. A military band approached, playing a gay martial air. It was at the head of a funeral, which seemed to be of a Mr. X; the casket rested on a draped gun carriage. She had a lively feeling of astonishment at the absurdity of making such an ado about the death of so insignificant a person. Behind followed the dead man's brother and one of his sisters, and behind them his two other sisters; they were incongruously dressed in a bright grey check. The brother advanced "like a savage," dancing and waving his arms; on his back was a yucca tree with a number of young blossoms.* This dream is a good example of the second of the three types mentioned above, being perfectly clear and yet apparently impossible to fit into the patient's waking mental life. The true meaning of it, however, became only too clear on analysis. The figure of Mr. X veiled that of her husband. Both men had promised much when they were young, but the hopes, their friends had built on them had not been fulfilled; the one had ruined his health and career by his addiction to morphia, the other by his addiction to alcohol. Under the greatest stress of emotion the patient related that her husband's alcoholic habits had completely alienated her wifely feeling for him, and that in his drunken moments he even inspired her with an intense physical loathing. In the dream her repressed wish that he would die was realised by picturing the funeral of a third person whose career resembled that of her husband's, and who like her husband, had one brother and three sisters. Further than this, her almost savage contempt for her husband, which arose from his lack of ambition and other more intimate circumstances, came to expression in the dream by her reflecting how absurd it was that any one should make an ado over the death of such a nonentity, and by the gaiety shown at his funeral not only by all the world (the gay air of the band; her husband is, by the way, an officer in the volunteers, while Mr. X has no connection with the army), but even by his nearest relative (the brother's dancing, the bright clothes). It is noteworthy that no wife appeared in the dream, though Mr. X is married, a fact that illustrates the frequent projection on to others of sentiments that the subject himself experiences but repudiates.

In real life Mr. X, who is still alive, is an indifferent acquaintance, but his brother had been engaged to be married to the patient, and they were deeply attached to each other. Her parents, however, manoeuvred to bring about a misunderstanding between the two, and at their instigation, in a fit of pique, she married her present husband, to her enduring regret. Mr. X's brother was furiously jealous at this, and the pæan of joy he raised in the dream does not appear so incongruous when we relate it to the idea of the death of the patient's husband as it does in reference to his own brother's death. His exuberant movements and "dancing like a savage" reminded the patient of native ceremonies she had seen, particularly marriage ceremonies. The yucca tree (a sturdy shrub indigenous to the Western States) proved to be a phallic symbol, and the young blossoms represented offspring. The patient bitterly regrets never having had any children, a circumstance she ascribes to her husband's vices. In the dream, therefore, her husband dies unregretted by any one, she marries her lover and has many children.

(4) The following two dreams illustrate the formation of neologisms. The patient, a woman of thirty-nine, dreamt that *she was sitting on a stage with four others, rehearsing a play they were to take part in; it seemed to be called "The Wreck of the Kipperling."* Her title rôle was called *Kipper*. She felt foolish and embarrassed. This feeling she had several times recently experienced, circumstances having placed her in an awkward and compromising situation in regard to a man and woman, for both of whom she cared. Years ago, when in school in France, she had greatly suffered from feeling awkward and silly at having to read aloud in class from French plays, a language she imperfectly pronounced. Three days before the dream she had been reading a volume of satirical poems by Owen Seaman, and being a foreigner had had considerable difficulty in understanding and appreciating them. This had distressed her, for her friends thought very highly of them. Her embarrassment culminated at the reading of one poem, in which Rudyard Kipling is depreciated and entitled "Kipperling;" she much admired Kipling's writings and had felt foolish when her two friends assured her he was crude and vulgar. She resented his being nicknamed Kipperling, and said "Fancy giving a poet the name of a silly little fish." From the fusion of Kipling and Kipperling, and perhaps influenced by the fact that the latter name had been employed by *Seaman*, she had coined for herself in the dream the title of *Kipper*. *Kipper* (fried herring) is frequently used in London slang to denote foolish people.

(5) In another dream the same patient imagined she was

called "*Hokerring*," a neologism produced by fusing "moke" (a London slang term for donkey, used under the same circumstances as Kipper) with "smoked herring;" this process may be represented thus:

(M) OKE
(SM) OKE (D)
H ERRING

(The parentheses indicate letters omitted in the neologism.) The term smoked herring reminded her of bloater, and of a rather vulgar word in her native language meaning nude, bloot (pronounced bloat). This brought up infantile memories of shyness and a sense of foolishness that were connected with nakedness.

The construction of the manifest content out of the latent content Freud terms the *Dream-making* (*Traumarbeit*). In this two other principal mechanisms are concerned in addition to those just mentioned of condensation and displacement. The first of these may be called *Dramatisation* (*Darstellung*). It is a familiar observation that the manifest content of most dreams depicts a situation, or rather an action, so that in this respect a dream may be said to resemble a theatrical representation. This fact exercises a selecting influence on the mental processes that have to be presented (*Rücksicht auf Darstellbarkeit*), for dramatisation, like the arts of painting and sculpture, is necessarily subject to definite limitations, and therefore special expedients have to be employed to indicate mental processes that cannot be directly portrayed. Just as a painter has indirectly to convey abstract mental processes by adopting certain technical devices, so a dramatist has to select and modify his material in order to make it conform to the restrictions of his art, as for instance when an action extending over years has to be presented in a couple of hours. In a dream the mental processes are dramatised so that the past and future are unrolled before our eyes in a present action; a wish, for instance, that relates to the future is seen realised in a present situation.

It is further well known that the manifest content of most dreams is predominantly, though not exclusively, of a visual nature, and the particular process of expressing in a dream various thoughts in the form of visual pictures Freud terms *Regression*, wishing to indicate by this the retrograde movement of abstract mental processes towards their primary perceptions. The network of dream thoughts is in this way resolved into its raw material. This process of regression is characteristic of dreams as contrasted with other mental constructions formed by means of similar mechanisms, such as day-dreams, psycho-neurotic symptoms and so on, though it sometimes occurs in the last named in the form of hallucinatory visions. In his discussion of the nature and function of

regression Freud develops a number of important theoretic considerations regarding the structure of the mind, which, however, cannot here be gone into. He traces regression, both in dreams and in visions, to the resistance of the censor of consciousness, and to the attraction exerted for the mental processes thus represented by infantile memories, which, as is known, characteristically preserve their original visual type. In the case of dreams, though not of course in the case of waking visions, it is possible that the regression is further facilitated by the cessation during sleep of the forward movement from the sensorial to the motor side.

Under the heading of dramatisation may also be included the representation of various intellectual processes. We shall presently see that the intellectual operations (judgement, etc.) that are frequently met with in the manifest content of dreams originate not in the dream-making but in the underlying dream thoughts; no intellectual work is performed in the dream-making proper. In the dream thoughts there are of course all kinds of intellectual processes, judgements, arguments, conditions, proofs, objections and so on. None of these, however, finds any special representation in the manifest content of the dream. As a rule they are entirely omitted, only the material content of the dream thoughts being represented in the dream, and not the logical relations of these. The dream-making, however, sometimes makes use of certain special devices to indicate these logical relations indirectly; the extent to which this is done greatly varies in different dreams and in different individuals. The logical relations between the constituents of the dream thoughts, just as between those of waking thoughts, are displayed by the use of such parts of speech as "if," "although," "either," "because," etc., which, as has just been said, find no direct expression in the manifest content. Instances of the devices in question are the following: Logical concatenation between two thoughts is indicated by the synchronous appearance of the elements representing these in the manifest content; thus, in the third dream related above, the husband's death, the second marriage and the subsequent children, three logically related thoughts, are represented by three groups of elements that synchronously appear in the manifest content. Causal connection between two dream thoughts is usually not indicated at all. When indicated it is done by making the one representing element follow on the other. The commonest way of doing this is by one clause being represented in an introductory dream (*Vortraum*), the other in the main dream (*Haupttraum*); it should however be remarked that this splitting of the manifest content does not always indicate causal connection between the corresponding dream thoughts.

A less frequent device is the bringing about a transformation of the one element into the other; the transformation must be a direct one, not a mere replacement, as when one scene passes gradually into another, not as when one scene is simply replaced by another. Evident absurdity in the manifest content signifies the existence of mockery or scorn in the dream thoughts, as was illustrated in the third dream related above. An alternative in the dream thoughts is not expressed in the manifest content; the representing elements are merely brought together in the same connection. When an alternative (either—or) appears in the manifest content it is the translation of "and" in the dream thoughts; thus in the second dream related above I felt that the third letter in the word outlined was either *v* or *d*, and both of these were present in the latent content.

Opposition and contradiction between dream thoughts may be indicated in two ways in the manifest content. When the contrasting thoughts can be linked with the idea of exchange, then the representing elements may be fused into a unity, a process described above under the name of identification. Other cases of opposition, which fall into the category of the converse or reverse, may be indicated in the following curious way; two parts of the already formed dream that are connected with the dream thoughts in question are inverted. Inversion of mental processes in dream-making subserves other functions than the one just mentioned; it is for instance a favourite method of increasing the distortion; the simplest way of disguising a mental process is to replace it by its obverse. Some subjects seem to employ this distorting mechanism to an inordinate extent, and many dreams can be interpreted merely by inverting them. The inversion may concern either space or time. An instance of the former occurred in the third dream related above, where the yucca tree (phallus) was attached dorsally instead of ventrally. Instances of both may be seen in the following dream by the same patient.

(6) *She stood at the seashore watching a small boy, who seemed to be hers, wading into the water. This he did till the water covered him and she could only see his head bobbing up and down near the surface. The scene then changed into the crowded hall of an hotel. Her husband left her, and she "entered into conversation with" a stranger. The second half of the dream revealed itself in the analysis as representing a flight from her husband and the entering into intimate relations with a third person, behind whom was plainly indicated Mr. X's brother mentioned in the former dream. The first part of the dream was a fairly evident birth phantasy. In dreams, as in mythology, the delivery of a child from the uterine waters is commonly presented by distortion as the entry of the child into water;*

among many others, the births of Adonis, Osiris, Moses and Bacchus are well known illustrations of this. The bobbing up and down of the head into the water at once recalled to the patient the sensation of quickening she had experienced in her only pregnancy. Thinking of the boy going into the water induced a reverie in which she saw herself taking him out of the water, carrying him to a nursery, washing him and dressing him, and installing him in her household.

The second half of the dream therefore represented thoughts, concerning the elopement, that belonged to the first half of the underlying latent content; the first half of the dream corresponded with the second half of the latent content, the birth phantasy. Besides this inversion in order, further inversions took place in each half of the dream. In the first half the child *entered* the water, and then his head bobbed; in the underlying dream thoughts first the quickening occurred, and then the child *left* the water (a double inversion). In the second half her husband left her; in the dream thoughts she left her husband.

Last among the dream-making mechanisms is that termed *Secondary Elaboration* (*secundäre Bearbeitung*). It fundamentally differs from the other three in that it arises from the activity, not of the underlying dream thoughts, but of the more conscious mental processes. This remark will be more comprehensible when we presently consider the forces that go to make a dream. When the dream is apprehended in consciousness it is treated in the same way as any other perceptive content, and is therefore not accepted in its unaltered state but is assimilated to pre-existing conceptions. It is thus to a certain extent remodelled so as to bring it, so far as is possible, into harmony with other conscious mental processes. In other words an attempt, however unsuccessful, is made to modify it so as to render it comprehensible (*Rücksicht auf Verständlichkeit*). This secondary elaboration is closely allied to the process I have described as rationalisation¹. As is well known, there is a pronounced tendency on the part of the mind to distort foreign experiences in such a way as to assimilate them to what is already intelligible; in hearing or seeing a sentence in a strange tongue the subject imagines analogies to familiar words in his own, a falsifying process that frequently is carried to excess, leading to curious misunderstandings. To this secondary elaboration is due whatever degree of ordering, sequence and consistency there may be found in a dream.

Reviewing now as a whole the process of dream-making we have above all to lay stress on the fact that in the formation of

¹*Journal of Abnormal Psychology*. June-July. 1908.

a dream no intellectual operation of any sort is carried out; the dream-making is concerned solely with translating into another form various underlying dream thoughts that were previously in existence. No creative work whatever is carried out by the process of dream-making; it performs no act of decision, calculation, judgement, comparison, conclusion or any kind of thought. Not even the elaboration of any phantasy occurs in the dream-making, though a previously-existing phantasy may be bodily taken over and woven into the dream, a fact that gives the key to the explanation of highly-wrought and yet momentary dreams such as the well known guillotine one related by Maury. Any part of a dream that appears to indicate an intellectual operation has been taken bodily from the underlying latent content, either directly or in a distorted form; the same applies to numerals and to speech phrases that may occur in a dream. Even some of the waking judgements passed on a dream belong to the latent content. To repeat, there is in the dream-making nothing but transformation of previously formed mental processes.

The dream-making proper is thus a process more distant from waking mental life than even the most determined detractor of dream activities would maintain. It is not merely more careless, incorrect, incomplete, forgetful and illogical than waking thought, but it is something that qualitatively is absolutely different from this, so that the two cannot be compared. Dream-making proceeds by methods quite foreign to our waking mental life; it ignores obvious contradictions, makes use of highly strained analogies, and brings together widely different ideas by means of the most superficial associations, for instance by such a feeble play on words as shocks the waking mind with a keen sense of ridiculousness. The mental processes characteristic of dreams would if they occurred in a conscious waking state at once arouse grave suspicion of impaired intelligence; as Jung has clearly pointed out¹ they are in fact processes that are frequently indistinguishable from those met with in advanced stages of dementia præcox and other psychoses.

The affect in dreams has many interesting features. The incongruous manner in which it may be present when it is not to be explained by the ideas of the dream, or be absent when from these ideas it might have been expected, has already been noted above, and is quite elucidated by psycho-analysis, which reveals that in the underlying dream thoughts the affect is logically justified and is congruous enough. The apparent

¹*Psychologie der Dementia præcox*, 1907. Translated by Peterson and Brill (*Journal of Nervous and Mental Disease*, Monograph Series, 1909).

incongruity is solely due to the distortion of the conceptual content, whereby a given affect becomes secondarily associated with an inappropriate idea. The third dream mentioned above well illustrates this fact; the incongruity with which Mr. X's death was joyfully celebrated by his brother explains itself as soon as one realises that the figure of Mr. X in the dream represented that of another man in the latent content. The affect investing the latent content is always more intense than that present in the manifest content, so that, although strongly affective dream thoughts may produce an indifferently toned dream, the reverse never occurs, that is to say an affective manifest content never arises from an indifferently toned latent content. Freud attributes this inhibition of the affect in dream formation partly to the cessation in sleep of the forward movement from the sensory to the motor side—he regards affective processes as essentially centrifugal—and partly to the suppressing effect of the censor, which will presently be further considered. Another important matter is that the nature of the affect as it appears in the manifest content is the same as that of the latent content, although, as has just been said, the intensity of it is always less there than here. The effect of the dream-making on the original affect is thus different from that on the rest of the dream thoughts, in that no distortion of it takes place. As Stekel puts it in a recent article¹, "*Im Traume ist der Affekt das einzig Wahre.*" The affect appears in the same form in the latent as in the manifest content, although through the mechanisms of transference and displacement it is in the latter otherwise associated than in the former. It should however be remarked that a given affect in the manifest content may represent its exact opposite in the latent content, but on closer analysis it will be found that the two opposites were already present in the latent content, and were both of them appropriate to the context; as is so often the case in waking mental life, exactly contrasting mental processes in dream thoughts are intimately associated with each other.

Having mentioned some of the mechanisms that bring about the distortion of the latent into the manifest content we may next shortly consider the material and sources from which a dream is composed. Again we have sharply to distinguish between the sources of the manifest content and those of the underlying dream thoughts; the latter will presently be dealt with apart. Three peculiar features shown by the memory in dreams have especially struck most observers: first the preference shown for recent impressions, secondly that the experiences are other-

¹*Jahrbuch f. psychoanalytische u. psychopathologische Forschungen.* Bd. 1, S. 485.

wise selected than in our waking memory, in that subordinate and hardly noticed incidents seem to be better remembered than essential and important ones, and thirdly the hypermnnesia for previously forgotten incidents, especially for those of early childhood life.

The first two of these features may be considered together, for they are intimately connected. In every dream without exception occur mental processes experienced by the subject in the last waking interval (*Traumtag*); other recent experiences that have not occurred on the day actually preceding the dream are treated in just the same way as more ancient memories. There must therefore be some special quality that is of significance in dream formation attaching to the mental experiences of the preceding day. Closer attention shows that the experience in question may be either psychically significant or quite indifferent; in the latter case, however, it is always associated with some underlying significant experience. The dream-instigator (*Traumerreger*) may be (1) a recent significant experience that is directly represented in the manifest content, (2) a recent significant experience that is indirectly represented in the manifest content by the appearance there of an associated indifferent experience, (3) an internal significant process (memory) that regularly is represented in the manifest content by the appearance of an associated, recent, indifferent experience. In each case, therefore, a recent experience (*i.e.* from the preceding day) appears directly in the dream; it is one either significant in itself or else associated with another (recent or old) significant one. The selection of incidents of subordinate interest applies only to incidents of the day before the dream. Older incidents, that at first sight appear to be unimportant, can always be shown to have *already* become on the day of their occurrence psychically significant through the secondary transference on to them of the affect of significant mental processes with which they have got associated. The material from which a dream is formed may therefore be either psychically significant or the opposite, and in the latter case it always arises in some experience of the preceding day.

The explanation Freud gives of these facts is shortly as follows. The meaning of the appearance in the manifest content of indifferent mental processes is that these are employed in the dream-making to *represent* underlying processes of great psychical significance, just as in battle the colours of a regiment, themselves of no intrinsic value, stand for the honour of the army. A more accurate analogy is the frequent occurrence in the psycho-neuroses of the transposition of a given significant affect on to an indifferent idea; for instance, in-

tense dread of a harmless object may arise as a transposition, on to the secondarily associated idea of this object, of a dread that was fully justified in relation to the primary idea. In short, the process is another form of the displacement mechanism described above. Just as in the psycho-neuroses, so also in the dream the primary underlying idea is of such a nature as to be incapable of becoming conscious (*bewusstseinsunfähig*), a matter that will presently be further discussed. Freud explains the regular occurrence in the dream of a recent experience by pointing out that this has not yet had time to form many associations, and therefore is more free to become associated with unconscious psychical processes. The circumstance is of interest as indicating that during sleep, and unnoticed by our consciousness, important changes go on in our memory and conceptual material; the familiar advice to sleep over an important matter before coming to a decision has an important basis in fact.

The third feature, namely the hypermnnesia particularly for experiences of early childhood, is of cardinal importance. Early memories, which the subject had completely forgotten, but the truth of which can often be objectively confirmed, not infrequently occur with startling fidelity even in the manifest content. This fact in itself should suggest the ontogenetic antiquity of dream processes. In the latent content the appearance of such forgotten memories is far more frequent, and Freud holds it probable that the latent content of every dream is connected with ancient mental processes that extend back to early childhood. The following instance may be given of this. (7) A patient, a man aged 37, dreamt that *he was being attacked by a man who was armed with a number of sharp weapons; the assailant was swarthy, and wore a dark moustache. He struggled and succeeded somehow in inflicting a skin wound on his opponent's left hand. The name Charles seemed to be related to the man, though not so definitely as if it were his name. The man changed into a fierce dog, which the subject of the dream succeeded in vanquishing by forcibly tearing his jaws apart so as to split his head in two.* No one could have been more astonished at the dream than the patient himself, who is a singularly inoffensive person. The name Charles led to the following free associations: a number of indifferent acquaintances having this as their Christian name—a man, named Dr. Charles Stuart, whom he had seen at a Scottish reunion, at which he had been present on the day before (this man, however, wears a beard)—another man present at the reunion whose personal appearance had many traits in common with his assailant in the dream—the Scottish Stuart Kings Charles I and Charles II.—again the acquaintance Charles Stuart—Crom-

well's designation of King Charles I., "that man Charles Stuart"—the medical practitioner of his family, whose name was Stuart Rankings, and who had died when the patient was nine years old. Then came the memory of a painful scene, previously quite forgotten, in which the doctor had roughly extracted two teeth from the terror-stricken patient after forcibly gagging his mouth open; before he could accomplish this the doctor had had his left hand badly bitten. The date of this occurrence could from extrinsic evidence be referred to the patient's fifth year. From a number of reasons that cannot be given here it became clear that the dream thoughts altogether clustered around this childhood experience. The assailant in the dream was no other than the doctor whose treatment of the patient was nearly thirty years after his death thus fearfully revenged in the latter's dream.¹ The play on his name Stuart Rankings (Rank-kings), which enabled him to become identified first with the Stuart King Charles, and then with Charles Stuart, and finally to be called in the dream plain Charles, is interesting. It should be added that the Dr. Charles Stuart mentioned above is a dental surgeon, who a week previously had in the patient's presence performed a painful tooth extraction on the latter's wife; on the day before the dream he had enquired of the patient concerning his wife's health. The identification of the man with the dog in the latter part of the dream was greatly over-determined. The doctor in question was a noted dog fancier, and had given the patient a fine collie to whom he became greatly attached; he led a very irregular life, and the patient often heard his father refer to him as a gay dog; finally he died "like a dog," from an accidental overdose of poison, in the presence of a number of people who were from ignorance powerless to render the slight assistance that would have saved his life.

The source of some dream material is to be found in somatic stimuli during sleep, though by no means so frequently as many writers maintain. They are, however, in no case the cause of the dream, but are merely woven into its fabric in exactly the same way as any other psychical material, and only when they fulfil certain conditions. The exaggerated claims sometimes made out for the importance of these stimuli are easily disproved by, for instance, the following considerations. A sleeper may react to a given somatic stimulation when this is of a lively nature, such as bad pain, in one of several different ways. In the first place he may altogether ignore it, as

¹The deeper interpretation of the dream will be easy to those familiar with psycho-analysis, especially when I add that the dream was accompanied by appalling dread, and that an early association to "hand" was "neck."

often occurs in bodily disease, secondly he may feel it during, or even throughout, sleep without dreaming at all, thirdly he may be awakened by it, and fourthly he may weave it into a dream. Even in the last instance it enters into the dream only in a disguised form, and it can be shown that this disguise depends on the nature not of the stimulus but of the rest of the dream. The same stimulus may appear in different dreams, even of the same person, under quite different forms, and analysis of the dream regularly shows that the form adopted is altogether determined by the character and motive of the dream. In short, the dream makes use of the somatic stimulus or not, according to its needs, and only when this fulfils certain requirements.

Having partly answered the question of *how* a dream is built we may take up the more difficult one of *why* it is built, or, more accurately put, the problems concerning the forces that go to make a dream. It is impossible to do this without first referring to Freud's views on psychical repression (*Verdrängung*) and unconscious mental processes; these views in themselves call for a detailed exposition which cannot here be given, so that this part of the present paper will be even more incomplete than the rest. Freud uses the term "conscious" to denote mental processes of which we are at a given moment conscious, "fore-conscious" (*vorbewusste*) to denote mental processes of which we can spontaneously and voluntarily become conscious (*e. g.*, a memory out of one's mind for the moment, but which can readily be recalled), and "unconscious" to denote mental processes which the subject cannot spontaneously recall to consciousness, but which can be reproduced by employing special devices (*e. g.*, hypnosis, psychoanalysis, etc.). He concludes that the force that has to be overcome in the act of making the last named processes conscious is the same as that which had previously opposed an obstacle to their becoming conscious, *i. e.*, had repressed them into the unconscious. This force or resistance is a defensive mechanism which has kept from consciousness mental processes that were either primarily or secondarily (through association and transposition) of an unacceptable nature; in other words these processes are inassimilable in consciousness. Returning now to the subject of dreams, we have first to remark that Freud empirically found an intimate and legitimate relation between the degree of confusion and incomprehensibility present in a given dream and the difficulty the patient experienced in communicating the free associations leading to the dream thoughts. He therefore concluded that the distortion which had obviously occurred in the dream-making was related to the resistance that prevented the unconscious dream

thoughts from becoming conscious; that it was in fact a result of this resistance. He speaks of the resistance that keeps certain mental processes unconscious as the "endopsychic censor." In the waking state the unconscious processes cannot come to external expression, except under certain abnormal conditions. In sleep, however, the activity of the censor, like that of all other conscious processes, is diminished, though it is never entirely abrogated. This fact permits the unconscious processes (the latent content) to reach expression in the form of a dream, but as they still have to contend with some degree of activity on the part of the censor they can only reach expression in an indirect way. The distortion in the dream-making is thus a means of evading the censor, in the same way that a veiled phraseology is a means of evading a social censor, which would not permit a disagreeable truth to be openly expressed. The dream is a compromise between the dream thoughts on the one hand and the endopsychic censor on the other, and could not arise at all were it not for the diminished activity of the latter during sleep.

Distortion of the dream thoughts by means of the mechanisms of condensation and displacement is far from being the only way in which the censor manifests itself, nor is this distortion the only way in which the censor can be evaded by the dream processes. In the first place we have already noticed above, one of its manifestations under the name of secondary elaboration. This process continues even in the waking state, so that the account of a dream as related directly after waking differs from that related some time after. The fact of this change in the subsequent memory of a dream is sometimes urged as an objection to the interpretation by psycho-analysis, but the change is just as rigorously determined and the mechanism is as precisely to be defined as those of any other process in the dream-making. For instance, if the two accounts are compared, it will be found that the altered passage concerns what might be called a weak place in the disguise of the dream thoughts; the disguise is strengthened by the subsequent elaboration by the censor, but the fact of the change points to the need for distortion at that given spot, a point of some value in the analysis. Instead of subsequently altering this weak place the censor may act by interposing doubt in the subject's mind as to the reliability of his memory about it; he may say "The person in the dream seemed to carry such and such an object, but I am not sure that I have n't imagined that in thinking over the dream." In such cases one is always safe in accepting the dubiously given point as unhesitatingly as the most vivid memory; the doubt is only one of the stages in the disguise of the underlying dream thoughts. An interesting way

in which the censor may act is by the subject receiving the assurance during the dream that "it is only a dream." The explanation of this is that the action of the censor has set in too late, after the dream has already been formed; the mental processes which have, as it were unwittingly, reached consciousness are partly divested of their significance by the subject treating them lightly as being "only a dream." Freud wittily describes this after-thought on the part of the censor as an *esprit d'escalier*. The last manifestation of the censor is more important, namely the tendency to forget dreams or part of them; it is an extension of the doubting process mentioned above. Freud traces this tendency to forget, as also that shown in many forgetting acts of waking life,¹ to the repressing action of the censor. This explanation can readily be experimentally confirmed. When a patient informs the physician that he had a dream the night before, but that he cannot recall anything of it, it frequently happens that the overcoming of a given resistance during the psycho-analytic treatment removes the barrier to the recollection of the dream, provided of course, that the resistance concerns the same topic in the two cases; the patient then says "Ah, now I can recall the dream I had." Similarly he may suddenly during the analysis of the dream, or at any time subsequent to the relation of the dream, supply a previously forgotten fragment (*Nachtrag*); this later fragment invariably corresponds with those dream thoughts that have undergone the most intense repression, and therefore those of greatest significance. This occurrence is extremely frequent, and may be illustrated by the following examples.

(8) A patient, a man aged 26, dreamt that *he saw a man standing in front of a hoarding, with a gate entrance on his left. He approached the man, who received him cordially and "entered into conversation" with him.* During the analysis he suddenly recalled that the hoarding seemed to be the wall of an "exhibition," into which the man was entering to join a number of others. The significance of this added fragment will be evident when I mention that the patient was a pronounced *voyeur*, and had frequently indulged in *pædicatio*.

(9) A patient, a woman aged 36, dreamt that *she was standing in a crowd of school girls. One of them said "Why do you wear such untidy skirts?" and turned up the patient's skirt to show how worn the underskirt was.* During the analysis, three days after relating the dream, the patient for the first time recalled that the underskirt in the dream seemed to be a nightdress, and analysis of this led to the evocation of several painful memories in which lifting a nightdress played an im-

¹ *Psychopathologie des Alltagslebens*. 3^e Ausg., 1910.

portant part; the two most significant of these had for many years been forgotten.

As was mentioned above, the censor can be evaded by the dream thoughts in other ways than the usual ones of distortion. They may appear in the manifest content in their unaltered form, but their significance be misunderstood by the subject when he recalls the dream. For instance, a person may dream that he sees his brother dead, the actual dream thoughts being the wish that the brother may die. The subject fails to realise that the picture corresponds with a wish, even a suppressed one, partly because the nature of this is so horribly unlikely that it does not occur to him, and partly because the dream is accompanied by an emotion, dread, which is apparently incongruous with a wish. Such dreams are always intensely distressing (*Angstträume*), and in a sense it may be said that the dread here replaces the distorting mechanisms of condensation and displacement.

We have finally to consider the most important problems of all, those relating to the latent content or dream thoughts. The first thing that strikes one about these is their intense psychological significance. A dream never proceeds from trifles, but only from the mental processes that are of the greatest moment and interest to the subject. "*Der Traum gibt sich nie mit Kleinigkeiten ab.*" The explanation of why incidents of apparently subordinate interest occur in the manifest content has been given above. More than this, the dream thoughts are processes of the greatest *personal* interest, and are thus invariably egocentric. We never dream about matters that concern others, however deeply, but only about matters that concern ourselves. It has already been mentioned that the underlying dream thoughts are perfectly logical and consistent, and that the affect accompanying them is entirely congruous to their nature. Freud, therefore, not only agrees with those writers who disparage the mental quality of dreams, holding as he does that the dream-making proper contains no intellectual operation and proceeds only by means of the lower forms of mental activity, but he also agrees with those other writers who maintain that dreams are a logical continuance of the most important part of our waking mental life. We dream at night only about those matters that have most concerned us by day, though on account of the distortion that takes place in the dream-making this fact is not evident. Lastly it may be added that all the dreams occurring in a given night arise from the same group of latent dream thoughts, though they often present different aspects of these.

There are certain differences between the dreams of a young child and those of an adult. In the child, at all events before

the age of four, no distortion takes place, so that the manifest content is identical with the latent content. In correspondence with this fact we find that child dreams are logical and co-ordinate, an observation that is hard to reconcile with the commonly received opinion that dream processes arise from a dissociated activity of the brain cells, for one can see no reason why dreams should be a meaningless conglomeration of disordered and lowered mental functioning in adults when they are obviously not so in the child. Further, with young children it is easy to recognise that the dream represents the imaginary fulfilment of an ungratified wish. Now Freud maintains that the latent content of every dream represents nothing else than the imaginary fulfilment of an ungratified wish. In the child the wish is an ungratified one, but it has not undergone repression, that is to say it is not of such a nature as to be unacceptable in consciousness; in the adult the wish is not merely one that could not be gratified, but is of such a nature as to be inassimilable in consciousness, and so has become repressed. It frequently happens that even in the adult a wish-fulfilment appears in the manifest content, and still more frequently that a wish-fulfilment not present in the manifest content, but revealed by psycho-analysis, concerns a wish of which the subject is quite conscious; in both these cases, however, full analysis always discloses that these wishes are merely reinforcements of deeper, unconscious ones of an associated nature. No wish, therefore, is able to produce a dream unless it is either unconscious (*bewusstseinsunfähig*) or else associated with an allied unconscious one.

It has sometimes been alleged by Freud's opponents that his generalisation of all dreams representing a wish-fulfilment is the outcome of observing a few child dreams, and that his analyses merely consist in arbitrarily twisting the dream, to serve some private ends, until a wish can be read into it. We have seen that this absurd suggestion is historically untrue, for Freud came to the analysis of adult dreams from the analysis, not of child dreams, but of adult psycho-neuroses. He found that his patients' symptoms arose as a compromise between two opposing wishes, one of which was conscious, the other unconscious, and that they allegorically represented the imaginary fulfilment of these two wishes. He further found that an essential factor in their production was a conflict between the two wish-systems, of such a kind that the unconscious one was forcibly prevented from becoming conscious; it was unconscious because it was repressed. It frequently happened that the psycho-analysis of the patients' symptoms directly led to their dreams, and on submitting these to the analysis in exactly the same way as any other mental material he discovered that the

construction of them showed close resemblances to that of the neurotic symptoms. In both cases the material examined proved to be an allegorical expression of deeper mental processes, and in both cases these deeper processes were unconscious, and had in reaching expression undergone distortion by the endopsychic censor. The mechanisms by means of which this distortion is brought about is very similar in the two cases, the chief difference being that representation by visual pictures is much more characteristic of dreams. In both cases the unconscious mental processes always arise in early childhood and constitute a repressed wish, as do all unconscious processes, and the symptom or dream represents the imaginary fulfilment of that wish in a form in which is also fused the fulfilment of the opposing wish.

Dreams differ from psycho-neurotic symptoms in that the opposing wish is always of the same kind, namely the wish to sleep. A dream is thus the guardian of sleep, and its function is to satisfy the activity of unconscious mental processes that otherwise would disturb sleep. The fact that sometimes a horrid dream may not only disturb sleep, but may actually wake the sleeper, in no way vitiates this conclusion. In such cases the activity of the endopsychic censor, which is diminished during sleep, is insufficient to keep from consciousness the dream thoughts, or to compel such distortion of them as to render them unrecognisable, and recourse has to be had to the accession of energy that the censor is capable of exerting in the waking state; metaphorically expressed, the watchman guarding the sleeping household is overpowered, and has to wake it in calling for help.

Freud couples with his discussion of dream problems a penetrating enquiry into many allied topics, such as the nature of the unconscious and the function of consciousness, that cannot here be even touched upon. I would conclude this imperfect sketch of his theory of dreams by quoting a remark of his to the effect that "*Die Traumdeutung ist die Via Regia zur Kenntniss des Unbewussten im Seelenleben.*"

THE PSYCHOLOGICAL ANALYSIS OF DREAMS

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It is not an uncommon phenomenon in the development of science that professional men of erudition, with all the help at their disposal, with all the implements of their knowledge and ability, combat some principle of popular wisdom which is, on the other hand, defended by the people with equal tenacity, and that finally science is forced to recognize that in essentials the popular conception, and not its own, is correct. It would be especially worthy of investigation to discover why it is that science, on its gradually mounting path, progresses in an irregular zigzag line, which at times comes close to the popular *Weltanschauung*, and then quite departs from it.

I mention this peculiar phenomenon for the reason that the latest investigations of dreams, those noteworthy and bizarre manifestations of mental life, have laid bare facts which compel us to abandon our former views of the nature of dreams, and, with certain limitations, to return to the popular theory.

The people have never given up a belief in the significance of dreams. The oldest writings which have been preserved to us, hewn out in stone in praise of the old Babylonian kings, as also the mythology and history of the Hindoos, Chinese, Aztecs, Greeks, Etruscans, Jews and Christians, take the point of view, held to-day by the mass of the people, that dreams can be interpreted. The interpretation of dreams was for thousands of years a special science, a particular cult, whose priests and priestesses often decided the fate of countries and called forth revolutions which changed the history of the world. This now antiquated science rested on the unshakable belief that the dream, though in a concealed way and by obscure analogies, was quite capable of interpretation by the initiate, revealed the future, and that by these nocturnal phenomena the higher powers desired to prepare mortals for approaching events of importance. In the lower ranks of the populace, the dream book, that noteworthy survival of ancient Babylonian astrology, still enjoys to-day great popularity and

¹Lecture given in the "Königliche Gesellschaft der Ärzte," in Budapest, Oct. 16, 1909. Translated from the German by Harry W. Chase, Fellow in Psychology, Clark University.

is much used. Although the details of the dream-books differ in essentials from each other in the different countries, they must be considered products of the common folk-spirit.

On the other hand, we find on the part of the great majority of recent psychologists an almost complete contempt for the dream as a psychic function, and as a result a denial also that the dream-content is of any significance. Many of these investigations consider the dream to be a senseless complex of hallucinations, which blaze up in a lawless way in the brain of the sleeper. According to the view of others, the dream is nothing but the psychic reaction to these outer (objective) or inner (subjective) stimuli, which the sensory end-organs of the body receive during sleep and conduct to the centres.

There were only a few who held the theory that the sleeping psyche could develop a complicated, significant activity, or that the dream could be maintained to have any sort of a symbolic meaning. But even these latter did not succeed in making the peculiarities of dreams comprehensible, without forcing their explanations into the Procrustian bed of an artificial playing with allegories.

Accordingly for centuries the army of superstitious interpreters of dreams stood over against that of the sceptics, until about ten years ago the Viennese neurologist, Professor Freud, discovered facts which make possible a unification of the two hostile conceptions, and which aided on the one hand in the discovery of the true nucleus in the age-old superstition, and on the other hand fully satisfied the scientific need of the knowledge of the connections between cause and effect.

I may say at this point, that Freud's theory of dreams and his method of interpretation only approach the popular conception in so far as to ascribe to dreams sense and meaning. But the newly discovered facts sustain in no way the belief of those who would base dreams on the interference of higher powers, and see prophecies in them. Freud's theory considers the dream as a mental product dependent upon endopsychic occurrences, and is not calculated to strengthen the belief of those who consider the dream as a device of higher powers or as the clairvoyance of the sleeper.

Psychoanalysis, a new method of investigation and treatment of psychoneuroses, made it possible for Freud to recognize the true significance of dreams. The method has its point of departure in the principle that the symptoms of these diseases are only the sensory images of particular thought-constellations, impregnated with feeling, which were distasteful to consciousness and therefore repressed, forgotten, but still live on in the unconscious; and in the fact that the surrogate-creations for the repressed material vanish as soon as the unconscious

thought can be brought to light and made conscious by help of free association. In the course of this analytic work the dreams of the patients were told, and Freud made their content also an object of psychoanalytic investigation. To his surprise he found in dream analysis not only a great aid to the treatment of neuroses, but he gained at the same time as a by-product a new explanation of the dream as a psychic function, more enlightening than any of the former explanations. In many chemical processes materials are incidentally obtained by the reduction of certain chemicals, which perhaps have been thrown away as useless for a long time, but which are shown after a time to be valuable materials, often surpassing the principal products of the manufacture in value. The case was about the same with the explanation of dreams incidentally found by Freud; it opens up such outlooks for the knowledge of both the sound and the diseased mind that in comparison its particular point of departure, the treatment of certain phenomena of nervous diseases, seems a scientific question of the second rank.

In the short time at my disposal I cannot reproduce exhaustively Freud's theory of dreams. I must rather confine myself to the more essential explanations and the most valuable facts of the new theory, and to its verification by examples. I do not imagine that this lecture will convince my hearers. According to my previous experience one can gain a conviction in affairs of psychoanalysis only for himself. So I shall not controvert here the lesser and quite superficial critics of Freud, but will rather explain in brief the most essential parts of the theory itself.

First a few words concerning method. If we desire to analyze a dream, we proceed exactly as in the psychological investigation of psychoneurotic symptoms. Behind each imperative thought, no matter how illogical it may appear, are hidden coherent but unconscious thoughts, and to make these evident is the problem of psychoanalysis. Freud has proved that the images and experiences of which the dream consists, are for the most part only disguises, symbolic allusions to suppressed trains of thought. Behind the *conscious dream-content* is hidden a *latent dream-material*, which, on its part, was aroused by *coherent, logical dream-thoughts*. The interpretation of the dream is nothing else than the translation of the dream from its hieroglyphic-symbolic speech into conceptual speech; the leading back of the obvious dream-content through the clues of association given by the hidden dream-material, to the logical dream-thoughts. The means by which this is done is the so-called free association. We have the dream related to us, divide the given material into several parts or sections, and

require the dreamer to tell all that occurs to him when he directs his attention, not to the whole of the dream, but to a definite part of it, to a particular event or word-image occurring in it. This association must, however, be *wholly* free, consequently the single thing forbidden is the dominance of critical choice among the irruptive ideas. Any halfway intelligent man can be brought to tell out all the thoughts associated with the fragments of the dream, whether clever or stupid, coherent or senseless, pleasant or unpleasant, suppressing the shame perhaps bound up with them. In this way also are worked over the other fragments of the dream and so we assemble the latent dream material, that is to say, all the thoughts and memories of which the conscious dream picture is to be considered the condensation-product (*Verdichtungsprodukt*). It is an error to believe that the activity of association when left free is devoid of any regulation by law. As soon as, in the analysis we disregard the conscious terminal idea (*Zielvorstellung*) of our thinking, the directive forces of the unconscious psychic activities prevail in the choice of associations, that is to say, just the same mental forces which functioned in the creation of the dream. We have been for a long time familiar with the thought that there is no chance in the physical world, no event without sufficient cause; on the ground of psychoanalytic experience we must suppose just as strong a determination of every mental activity, however arbitrary it may seem. It is therefore an unjustified fear that the activity of association when freed from all restraints in such analysis, will give results which have no value. The subject of the analysis, who at first reproduces his apparently senseless ideas with scornful scepticism, soon discovers, to his own surprise, that the train of associations, uninfluenced by conscious aids, leads to the awakening of thoughts and memories which were long since forgotten, or repressed on account of the pain they caused. But through the emergence of these the fragment taken from the dream is made intelligible or capable of interpretation. If we repeat this process with all the parts of the dream, we see that the trains of thought which radiate out from the different fragments converge in a very essential train of thought, which was stimulated the day before the night of the dream—the dream thoughts themselves. Once these are recognized, not only the single fragment, but also the dream as a whole seems coherent and intelligible. If, finally, we compare the point of departure of the dream, the dream thoughts, with the content of the naïvely related dream, we see that *the dream is nothing else than the concealed fulfilment of a repressed wish*.

This sentence contains the most essential results of Freud's investigation of dreams. The idea that the dream fulfills

wishes, which in the rude world of facts must be unfulfilled, seems to share in the language of abandoned popular science. "Dreams" are used metaphorically in most languages for "wishes" and the Hungarian proverb says just this, that "swine dream of acorns, the goose of maize"—which is only to be regarded as an allusion to the similar direction of human dreams.

Some of the dreams of adults and most of the dreams of children are purely wish-fulfillment dreams. The child dreams of pleasureable experiences denied him by day, of the toys which he envied his little comrades, of victorious struggles with those of his own age, of his good mother, or his friendly father. Very often in his dream he seems "big," endowed with all the freedom and power of his parents, which he wishes for so ardently by day. Wish dreams like these also occur to adults. The difficult test (about which we are so anxious) seems in dreams splendidly passed, dear relatives awaken from their graves and assure us that they are not dead, we appear to ourselves rich, powerful, endowed with great oratorical gifts, the most beautiful of women solicit our favor, and so on. For the most part we attain in dreams just that which we painfully miss on waking.

The same tendency to wish-fulfillment rules not only nocturnal, but day dreams as well, the fancies in which we can catch ourselves at unoccupied moments or during monotonous activity. Freud has observed that women's fancies deal for the most part with things which immediately or mediately belong to the sex life (of being loved, proposals, beautiful clothes), those of men predominantly with power and esteem, but also with sexual satisfaction.

Fancies concerning the means of escape from a real or imagined danger and the annihilation of real or imagined enemies are also very common. These simple wish-fulfillment dreams and fancies have an obvious meaning, and need no particular labor for their interpretation.

But that which is new, surprising and incredible to many in Freud's explanation of dreams is the assertion that *all* dreams, even those which seem indifferent or even unpleasant, can be reduced to this basal form, and that it can be shown by analysis that they fulfill wishes in a disguised way. In order to understand this, we must first make ourselves familiar with the mechanism of psychic activity in dreams.

The associative analysis of a dream is only the reversal of the *synthetic* work which the psyche executes at night while it transforms the unwelcome thought and the unpleasant sensation which disturb sleep into wish-fulfilling dream-images. By a critical consideration one is convinced that this work never ceases during sleep, even when after waking we cannot recall

that we dreamed at all. The traditional idea that dreams disturb rest during sleep must be abandoned on the ground of these newly won results; on the contrary, since they do not allow the unpleasant, painful or burdensome thought which would disturb sleep to become conscious with its true content, but in a changed form as the fulfillment of a wish, we are forced to recognize dreams as the preservers of sleep.

The psychic factor which watches over rest during sleep, often with the assistance of the dream disguise already mentioned, is the censor. This is the gate-keeper at the threshold of consciousness, which we see zealously at work during waking life also, especially in psychoneuroses, and which for our problem is to be considered as either repressing all thought groupings which are distasteful in æsthetic or ethical ways, or disguising them in the form of apparently harmless symbols, symptomatic acts or symptomatic thoughts.

The function of the censorship is to secure the repose of consciousness and to keep at a distance all psychic creations which would cause pain or disturb rest. And like the censor of political absolutism, who sometimes works at night, the psychic censorship is kept in activity during sleep, though its red penciling is not so strongly in evidence as in waking life. Probably the censor is led to relax its activity by the idea that the motor reactions are paralyzed during sleep, and so thoughts cannot be expressed in deeds. So the fact may be explained that for the most part those images and situations emerge as wish fulfillments in dreams which we refuse by day to recognize as wishes.

We all shelter in our unconscious ego many wishes repressed since childhood, which take the opportunity of exercising their psychic intensity as soon as they perceive the letting down of the censorship at night.

It is not chance that among the wishes revealed in dreams, the strongly repressed sexual excitations, and in particular those of the most contemptible kind play the greatest rôle. It is a very great error to believe that psychoanalysis intentionally places sexual activity in the foreground. It cannot be denied that whenever one seeks to investigate thoroughly the basal facts of mental life, he always strikes against the sexual elements. If, accordingly, we find psychoanalysis objectionable for this reason, we are really degrading the description of the unconscious facts of human mentality by our action in regarding them as obscene. The censorship of affairs of sex is, as already said, much milder in dream life than during waking hours, so that in dreams we experience and crave sexual experiences without bounds, even representing in our dreams experiences and acts reminding one of the so-called

perversions. I avail myself as an example of the dream of a patient who was extraordinarily modest in waking life. He saw himself enveloped in an antique peplum, fastened in front with a safety-pin; suddenly the pin fell out, the white garment opened in front, and his nakedness was admired by a great crowd of men. Another, equally modest patient, told me this, which is an exhibition dream with somewhat altered circumstances: She was enveloped from top to toe in a white garment and bound to a pillar; around her stood foreign men, Turks or Arabs, who were haggling over her. The scene reminds one very strongly, apart from her enveloping garment, of an Oriental slave market; and, indeed, analysis brought out that this lady, now so modest, when a young girl had read the tales of the "Thousand and One Nights," and had seen herself in fancy in many of the situations of the highly colored love scenes of the Orient. At that time she imagined that slaves were exposed for sale not clothed, but naked. At present she repudiates the idea of nudity so strongly even in dreams that the suppressed wishes which bear upon this theme can only come into being *when changed to their opposite*. A third dreamer only allowed herself so much freedom in this respect as to move about in the midst of the other forms of her dream incompletely clad, in her stockings or with bare feet; and here analysis showed that as a child for a long time she enjoyed greatly removing her clothes and going without them, so that she was nicknamed "the naked Pancrì" (her name was Anna, in Hungarian Pauna). Such exhibition dreams are so frequent that Freud was able to put them in the class of his "typical dreams," which recur with most people from time to time and have the same origin. They are based on the fact that there is living on in all of us an undying longing for the return of the paradisaical conditions of childhood; this is the "golden age," that poets and Utopians project from the past into the future. It is a very common means of dream disguise to circumvent the censorship by presenting the wish not as such, but only in the form of an allusion in the dream. It would not be possible to understand, for example, why one of my patients dreamed so often of sexual scenes with a man by the name of Frater, who was quite indifferent to her, if we had not learned that in her youth her brother (*frater*) was her ideal and that in childhood the affection of the pair had often taken on a purely erotic form and manifested itself by relations that she now repressed as incestuous. This repression of forbidden things often enters into the dream, especially with persons who in consequence of incomplete satisfaction of the libido are inclined to the development of anxiety (Freud's anxiety-neurotics). Nocturnal anxiety can become so great

that the dreamer awakens with feelings of pain (*pavor nocturnus*). Anxiety which has a physiological basis, gives in such cases an opportunity for the deeply repressed childish-perverse excitations to involve themselves in the dream, in the form of frightful, cruel, horrible scenes, which seem frightful to us, but in a certain depth of the unconscious satisfy wishes which in the "prehistoric" ages of our own mental development were actually recognized as desires.

The great part played in such dreams by cruelties inflicted or suffered must find its explanation in the sadistic idea which children have of the sex-relationship, as Freud has so beautifully shown in his "infantile sexual theories."¹ All the cruel acts of such dreams appear in analysis as sexual events transformed into deeds of violence. Sexually unsatisfied women, for example, very commonly dream of thieves breaking in, of attacks by robbers or wild beasts; but not one of the concealed, well-hidden incidents of the dream betrays that the outrages to which the dreamer is subjected really symbolize sexual acts. An hysterical of my observation once dreamed that she was run down by a bull before which she held a red garment. There was involved in this dream not only the present wish to possess such a dress, but also an unavowed sexual wish, the same which also caused the sickness. The thought of the frightfully enraged bull, which is a widespread symbol of manly strength, came to her especially through the circumstance that a man with a so-called "bull neck" had played a certain rôle in the development of her sexual life.

Childish memories make continual and always significant contributions to the creation of the dream. Freud has also established the fact that the earliest age of childhood is not only not free from sexual excitations, but that rather infantile sexuality, not yet restrained by education, is expressly of a perverse character. In infantile sexuality the oral and anal-urethral erogeneous zones, the partial impulses (*Partialtriebe*) of sexual curiosity and of exhibitionism, as well as sadistic and masochistic impulses rule. When we consider these facts we come to the conclusion that Freud is in the right when he says that dreams express such impulses as wish-fulfillments, as the fulfillment of wishes from that part of our childhood which seems long since outgrown.

There are, however, dreams of very unpleasant content, which peculiarly enough disturb our rest hardly at all, so that when we awaken we reproach ourselves for experiencing such terrible events with so little sympathy or feeling. This sort of

¹S. Freud: Sammlung kleiner Schriften zur Neurosenlehre, II Folge. Deuticke, Vienna, 1909.

dream was observed, for example, by one of Freud's patients, who in a dream was present at the funeral of a beloved nephew. An apparently unessential detail of the dream, a concert ticket, led to the explanation of such an occurrence. The lady intended to attend a concert on the next night, where she expected to see again the man whom she formerly loved and had not yet forgotten, whom she had met for the last time a long while before, at the funeral of another nephew. So the dream, in order to hasten the meeting, sacrificed the other nephew. But the censorship, seemingly knowing that by the dream a harmless wish, and not that of death, was to be fulfilled, let the funeral "pass," without attaching to it any obvious emotional excitation. This analysis may serve as an example for all those dreams which apparently contradict Freud's wish theory, and which have to do with very unwelcome things or even with the non-fulfillment of wishes. If we seek out the latent dream thoughts concealed behind these painfully toned dreams, it becomes clear to us that, as Freud himself expresses it *the non-fulfillment of a wish in a dream always means the fulfillment of some other wish.*

When we consider the materials of the dream gained by free association from the conscious dream elements, it becomes clear that they flow pretty generally from two opposed sources; from childhood memories on the one hand, and from unobserved experiences of the "dream day," often quite indifferent, which were not reacted to. Indeed, according to Freud's expression, every well-articulated dream stands as it were on two legs, and is shown by analysis to be *over-determined* (*Ueberdeterminiert*), that is, to be the fulfillment of both a present and a long suppressed wish.

As an example I may relate the dream of a patient suffering from a nervous difficulty in urination. "A polished floor, wet, as though a pool lay there. Two chairs leaning against the wall. As I look around, I note that the front legs of both chairs are missing, as when one wants to play a joke on some one and has him sit down on a broken chair, and he falls. One of my friends was also there with her affianced."

Free association on the theme of the polished floor gave the fact that on the day before her brother in a rage had thrown a pitcher to the floor, which, with the water spilled over it, looked like the floor in the dream. She also remembered a similar floor from her childhood. On this occasion her brother, who was then very young, had made her laugh so hard that micturition ensued. This part of the dream, which also proved significant for the symptom-creation of the neurosis, accordingly fulfilled infantile erotic wishes which could now in consequence of strong censorship be presented only in allusions.

The two broken chairs leaning against the wall were shown by analysis to be a scenic presentation of the proverb "To fall to the ground between two stools" (that is, to be deceived from two directions). The patient had already had two suitors, but the family constellation already mentioned (the unconscious love for her brother) prevented the marriage on both occasions. And although her unconscious ego, according to her repeated testimony, had long been reconciled to the thought of spinsterhood, she still seems in the depth of her soul to have regarded with some envy the recent betrothal of one of her friends. The affianced couple had in fact been calling on her the day before.

According to Freud's theory we can picture to ourselves the origin of this dream in the following way: The dream-work succeeded in uniting two experiences of the day before, the breaking of the pitcher and the visit of the betrothed pair, with that train of thought, always emotively toned, which, though already suppressed in childhood, was always in condition to lend its affective energy to any present mental image which could be brought into even a superficial connection with it. Freud compares the dream to the promotion of a business undertaking, in which the unconscious, suppressed complex furnishes the capital, that is, the affective energy, while the wishes play the rôle of promoters.

Another source of dreams is in those sensory and sensible nerve-stimulations to which the organism is subject during sleep. These may be: dermal stimuli, the pressure of mattress and covering, cooling of the skin; acoustic or optical stimuli to which the sleeper is subjected; organic sensations: hunger, thirst, an overloaded stomach; a stimulated condition of the sex parts, and so on. Most psychologists and physiologists are inclined to attribute too great significance to stimuli of this sort; they think that they have given a satisfactory explanation of all dreams when they say that the dream is nothing but the sum of such psychophysics reactions, set free by nerve stimuli of this character. On the other hand, Freud rightly says that the dream does not admit these bodily stimuli as such to consciousness, but disguises and alters them in particular ways; the motive and means of this disguise are given not through the outer stimuli, but from mental sources of energy. The nerve stimuli during sleep offer then, as it were, only the opportunity for the unfolding of certain immanent tendencies of the psychic life. Analysis shows that dreams caused by nerve stimulation are also open or concealed wish-fulfillments: the thirsty man drinks great amounts of water in his dreams, the hungry man satisfies himself; the sick man who is disturbed by the ice-packing on his head throws it away, for he thinks

of himself in his dream as already well; the painful throbbing of a boil on the perinæum leads to the dream idea of riding. So it is made possible that the hunger, the thirst, the pressure on the head, the painful inflammation, do not waken the sleeper, but are transmitted into wish-fulfillments by the psychic forces.

The anxiety-dream known as "*Alpdruck*" brought on by an overloaded stomach, respiratory disturbances or disturbances of the circulation, or by intoxication, permits of explanation in the same way; the unpleasant bodily sensations offer an opportunity for deeply repressed wishes to fulfill themselves, wishes which the censorship of culture will not allow to pass and which can break into consciousness only in connection with feelings of anxiety and disgust.

In the process of analysis, as already said, we retrace, only in the reverse direction, the same way which the sleeping soul has travelled in the creation of the dream. And when we compare the manifest dream, often very short, with the rich material which is brought to light during the process of analysis, and when we consider that in spite of this quantitative difference all the elements of the latent dream-content are contained in some way in the portion which is manifest to us, we must agree that Freud is right when he considers this dream-condensation as the most toilsome part of the creation of the dream. I will attempt to show this by an example. A patient suffering from psychosexual impotence brought to me on one occasion a dream made up of two fragments. In the first fragment the only occurrence was that instead of a Hungarian paper, "*Pesti Holap*," which regularly came to him, he received the Vienna "*Neue Freie Presse*," to which as a matter of fact one of his colleagues subscribed. The second fragment of the dream dealt with a brunette, whom he wanted to marry at any price. It turned out that he acquired in his dream not the foreign paper, but in the hidden sense of the dream, a foreign woman, to whom in fact a colleague had "subscribed." This woman had long excited his interest, for it seemed to him that just this person could bring to functioning his sexuality which was struggling with strong inhibitions. The thought associations which came from this idea made it clear that he had been deceived in his hopes of another woman, with whom, with the same object in view, he had entered into a lasting relation. This woman, since she was a Hungarian, he had concealed in the dream behind the name of the paper "*Pesti Holap*." Of late he had occupied himself in seeking *free* sexual associations, which led to no obligations, instead of such more stable relations. When we know the great freedom with which the dream avails itself of symbols, we are not surprised to learn that my patient also applied the word "*Press*" in a sexual sense.

The second part of the dream shows, as though it wished to confirm our interpretation, that the patient had often been obliged to think, not without anxiety, that relations which lasted too long, like that between himself and his friend, could easily lead to a *mésalliance*. One who does not know that, as Freud has shown in his monograph, the psychic motive and means of presentation of wit are almost exactly the same as those which come out in dreams,¹ might consider us guilty of cheap wit in saying that the dream succeeds in condensing in the words "Neue Freie Presse" all the patient's thoughts and wishes which concerned themselves with the pleasures of which his sickness had robbed him, and the means of benefit which he had in mind, namely, the stimulus of the new and the greater freedom for which he was striving. (Novelty and journal are expressed in Hungarian by the same word "*ujzag*."')

Very characteristic products of the dream-condensation are the composite images (*Mischbildungen*) of persons, objects and words. These "monstrosities of the dream world" have contributed largely to the fact that dreams up to our day have been regarded as productions of the mind which were without value and without sense. But psychoanalysis convinces us that when the dream links together or fuses two features or concepts, it furnishes a less successful product of the same work of condensation to which the less obvious parts of the dream owe their disguise. One of the rules of the art of dream interpretation states that in cases of such composite images the dream material of the single constituents must first be sought, and then only can it be determined on what basis of a common element or similarity the welding together has taken place. An example of this, which is theoretically valuable, I owe to one of my patients. The composite picture which occurred in one of her dreams, was made up of the person of a physician and of a horse, which in addition was attired in night clothing. Associations led from the horse into the childhood of the patient. She suffered as a girl for a long time from a pronounced phobia of horses; she avoided them particularly on account of their obvious and open satisfaction of their bodily needs. In addition it occurred to her that as a child she had often been taken by her nurse to the military quarters, where she had had the opportunity to observe all these things with a curiosity which was at that time still unrestrained. The night-clothing reminded her of her father, whom she had had the opportunity to see, while she still slept in the room of her parents, not only in such costume, but in

¹S. Freud: *Der Witz und seine Beziehung zum Unbewussten*, Deuticke, Vienna, 1909.

the act of satisfying his bodily needs. (This is a frequently repeated case; parents for the most part put no restraint on themselves before three and four-year old children, whose understanding and faculty of observation they materially underestimate.) The third constituent of the composite picture, the physician, awakened in me the suspicion, which proved to be well grounded, that the patient had unconsciously transferred her sexual curiosity from her father to the physician who was treating her.

Many times the constituent parts of a composite person have an unequal share in its creation; perhaps only a characteristic movement of one person is grafted on to the second person. I saw myself once in a dream rub my forehead with my hand just as my honored master, Professor Freud, does, when he is meditating over a hard question. It does not require much art of interpretation to guess that this mixing of teacher and pupil, especially in meditation, can only be blamed to envy and ambition, when at night the intellectual censorship was relaxed. In my waking life I have to laugh at the boldness of this identification, which strongly reminds one of the sentence, "How he clears his throat and how he spits, that have you bravely learned from him." As an example of a composite word, I may say that in a dream a German speaking patient thought of a certain Metzler or Wetzler. Persons with such names are, however, unknown to the patient. He was, however, very much occupied on the day before with the approaching marriage of a friend, by the name of Messer, who liked to joke (*hetzen*)—South-German for *necken*—with the patient. The associations from Messer showed that he as a small child had been greatly in fear of his grandfather, who, while whetting (*wetzen*) his pocket-knife (*Taschenmesser*) had jokingly threatened him with castration, a threat which was not without influence on the development of his sexuality. The name "Metzler-Wetzler" are accordingly nothing but condensations of the words *messer*, *hetzen* and *wetzen*.

Dream condensation stands in close relation with the work of displacement and transvaluation (*Verschiebungsarbeit*) of the dream. This consists essentially in the fact that the psychic intensity of the dream thoughts is shunted over from the essentials to the accessories, so that the thought-complex which is at the focus of interest is represented in the conscious dream content either not at all or by a weak allusion, while the maximum of interest in the dream is turned to the more insignificant constituents of the dream thoughts. The work of condensation and displacement go hand in hand. The dream makes harmless an important thought, which would disturb the rest of the sleeper, or be censured on ethical grounds. It as it were

goes beyond such a thought, by attaching to its more unessential parts memory images until the condensed psychic intensity of these latter is able to distract the attention from the particularly interesting thought. As an example of the displaced centre of the conscious dream in comparison with the centre of the dream thoughts I may mention the already cited dream of the aunt concerning the death of her beloved nephew. The actually non-essential funeral took up the largest place in the dream, the personality which was very significant for the dream thoughts was on the contrary present in the dream only through a distant allusion.

I was once called upon to analyze the very short dream of a woman; she had wrung the neck of a little barking, white dog. She was very much amazed that she, who "could not hurt a fly" could dream such a cruel dream; she did not remember having had one like it before. She admitted that she was very fond of cooking and that many times she had with her own hands killed chickens and doves. Then it occurred to her that she had wrung the neck of the little dog in her dream in exactly the same way that she was accustomed to do with the doves in order to cause the birds less pain. The thoughts and associations which followed had to do with pictures and stories of executions, and especially with the thought that the executioner, when he has fastened the cord about the neck of the criminal, arranges it so as to give the neck a twist, to hasten death. Asked against whom she felt strong enmity at the present time, she named a sister-in-law, and related at length her bad qualities and the malicious deeds, with which she had disturbed the family harmony, before so beautiful, after insinuating herself like a *tame dove* into the favor of her later husband. Not long before, there had taken place between her and the patient, a very violent scene, which ended by the patient showing the other woman the door with the words: "Get out; I cannot endure a biting dog in my house." Now it was clear whom the little white dog represented, and whose neck she wrung in her dream. The sister-in-law is also a small person, with an extraordinarily white complexion. This little analysis enables us to observe the dream in its displacing and so disguising activity.

Without doubt the dream has used the comparison with the biting dog, instead of the real object of the execution-fancy (the sister-in-law), smuggling in a little white dog, just as the angel in the Biblical story gave Abraham a ram to slaughter at the last instant, when he was preparing to sacrifice his son. In order to accomplish this, the dream had to heap up memory images of the killing of animals until by means of their condensed psychic energy the image of the hated person paled,

and the scene of the obvious dream was shifted to the animal kingdom. Memory images of human executions serves as a connecting link for this displacement.

This example gives me the opportunity to repeat that, with few exceptions, the conscious dream-content is not the true reproduction of our dream thoughts, but only a displaced wrongly accented caricature, whose original can only be re-constructed by the help of psychoanalysis.

It is a noteworthy phenomenon of dream work that the material of abstract thought, the concept, is capable of presentation in the dream only little or not at all, that rather the dream as it were *dramatizes* thoughts only in optic or acoustic sense-images, changes them to scenes enacted on a stage, and in this way brings them to presentation. Freud characterizes very strikingly the difficulty which this necessity of working only with concrete material imposes upon the dream, when he says that the dream itself has to turn the thoughts of a political editorial into illustrations.

Dreams are fond of using ambiguous words and interpretations of all sorts of expressions in concrete or metaphorical senses in order to make abstract concepts and thoughts capable of presentation and so of inclusion in the dream.

The memory of every halfway educated man contains a large number of proverbs, quotations, modes of expression, parables, fragments of verse and so forth. The content of these offers an ever present, very suitable material which can be applied to the scenic presentation of a thought or to an allusion to it. I will attempt to explain this by a series of examples. One of my patients related to me the following dream; "I go into a great park, walking on a long path. I cannot see the end of the path or of the garden hedge, but I think I will go on until I arrive at the end."¹ The park and hedge of the dream resembled the garden of one of her aunts, with whom she had passed many happy holidays of her youth. She remembers in connection with this aunt that they customarily shared the same chamber, but when her uncle was at home the guest was "put out" into a neighboring room. The girl at that time only had a very fragmentary conception of the affairs of sex, and tried often by peeping through cracks in the door and through the keyhole, to find out what was going on within.

The wish to get to the end of the hedge symbolized in this dream the wish to get to the bottom of what was going on between the married pair. This wish was moreover determined by an experience of the day before.

¹ A note by the author at this point explains that the dream was in the Hungarian language, and the inference is that the sense of the dream depends on a Hungarian idiom which evidently has no correlate in German, and which is not given by the author. TR.

Another patient dreamed of the corridor of the girls' boarding school in which she was educated. She saw her own closet there, and desired to open it, but could not find the key, so that she was forced to break the door. But as she violently opened the door, it became evident that there was nothing within. The whole dream proved to be a symbolic masturbation-phantasy, a memory from the time of puberty; the female genitals were, as so often happens, presented as a closet. But the supplement to the dream, "there is nothing within" (*es ist nichts darin*) means in the Hungarian language the same as the German expression "it is no matter" (*es ist nichts daran*), and is a sort of exculpation or self-consolation of this sufferer from a bad conscience.

Another girl, whose neurosis was brought on by the death of her brother, who, according to her view, married too early and was not happy in his marriage, dreamed continually of the dead man. Once she saw him lying in his grave, but the head was turned to one side in a peculiar manner, or the skull had grown to a bough; another time she saw him in his childhood dress on an elevation from which he had to jump down. All this symbolism was a complaint against the wife and the father-in-law of the dead man, who turned the boy's head, when he was almost a child, and in the end made him "jump down" (which is a pure Hungarian idiom), and with all that did not consider him as their equal, for they once called him, referring to his modest origin, "one fallen from a bough" (again a Hungarian idiom).

Very often falling from a great height pictures in a concrete way the threat of ethical or material fall; with girls sitting may mean spinsterhood (*Sitzenbleiben*); with men a great basket may mean the fear of an unsuccessful wooing. It occurs still more commonly that the human body is symbolized by a house, whose windows and doors symbolize the natural openings of the body. My patient who suffered from sexual impotence made use of a trivial Hungarian expression for coitus, namely the word "to shoot," and dreamed very often of shooting, missing fire, the rusting of his firearms, and so forth.

It would be an enticing problem to assemble the fragments of dreams which can be explained symbolically and to write a modern dreambook, in which the explanation could be found for the separate parts of dreams. But this is not possible, for although much typical material recurs in dreams and in most cases can be rightly explained without analysis, symbols may have different meanings with different individuals, and even with the same individual at different times. Accordingly, if we wish to know in any particular case all the determinants of the single dream fragments there is nothing left for us but

painful analysis, for which the investigating power and the wit of the interpreter of the dream alone will not suffice, but the industrious co-operation of the dreamer is indispensable.

Still greater difficulties than are created by the presentation of abstract thoughts are met when the dream attempts to present in a concrete way the thought relations of the single dream-thoughts. It is Freud's valuable service to have succeeded in making it possible to discover the whole of the hidden formal peculiarities of the articulation of the dream, with which the dream attempts to present logical relations. Logical relations between two dream elements with respect to the dream-thoughts which are hidden behind them, are presented in the simplest case by temporal, spatial proximity, or by a fusion of the features of the dream.

The dream lacks a means of presentation of causal connection, of the either-or relation of conditions, and so forth, so that all these relations are brought to presentation in a very insufficient way by means of a temporal sequence of the dream elements. From this fact arise many embarrassments for the interpreter of dreams, and often only the communications of the dreamer can extricate him. But much may be guessed. For example, if a dream picture changes to another, we can divine behind this, cause and effect; but this connection the dream often presents by two completely separated dreams, one of which signifies the cause, the other the effect. Even in the presentation of a simple negative the dream can succeed only with great difficulty, so that—as we know from Freud—we can never tell in advance whether the dream thought is to be interpreted in a positive or a negative way. Considering the complexities of our mental organism it is only too easily seen that affirmation and negation of the same thoughts and feeling-complexes is to be met with in the dream thoughts side by side, or, rather in succession. It may be taken as a sign of displeasure and scorn when anything in a dream is presented in a reversed form, or when the truth is presented very openly and in a striking way. The feeling of inhibition, which is so common, signifies a conflict of the will, the struggle of opposing motives.

Now in spite of the lack of all logical relations in the change of the dream thoughts into the manifest dream, the latter often seems to be possessed of sense and to be correlated. When this is the case, it may result from one of two causes. We may be concerned on the one hand with a *dream-phantasy*, that is, with the reproduction of fancies which have grown up in the waking life, articles read in books or journals, fragments of romances or bits of conversation spoken or heard by the person himself. A deeper and more general explana-

tion for the apparently logical articulation of many dreams is, however, the fact that the rationalizing tendency of mental activity, which seeks to arrange senseless material into logical trains of thought, does not rest at night. This last activity of the dream Freud calls the *secondary working-over* (*sekundäre Bearbeitung*). It is due to this that the originally fragmentary parts of the dream are rounded out to a whole by supplementarily inserted connecting words and other little connections.

Because the dream has fundamentally condensed, displaced, disguised, scenically presented a dream thought, robbed it of its logical connections and worked it over in a secondary way, the work of interpretation is often very difficult. We are confronted by the conscious dream-content as by a hieroglyph or by a rebus which is very difficult of solution, and as a result the explanation of many dreams needs, besides the rules of Freud's interpretation, an especial capacity and inclination to occupy oneself with questions of the mental life.

Not less a riddle than the dream itself is its rapid fading out after awakening. When we awake the dream images so toilsomely built up at night collapse like a house of cards. During sleep the mind is like an air-tight room, into which neither light nor sound can penetrate from without, but within its own walls the slightest sound, even the buzzing of a fly, can be heard. But awakening is like opening the door to the air of the bright morning; through the doors of our senses press in the bustle and the impulses of everyday life, and the daily cares, lately soothed to sleep by wish-fancies, again take up their reign. The censor, too, awakes from its slumber, and its first act is to declare the dream to be "foolishness," to explain it as senseless, to put it as it were under guardianship. It is not always satisfied with this measure, it reacts much more strongly against the revolutionary dreams (and there is not a single dream which cannot be shown by analysis to offend against some ethical or legal canon). The stronger method consists in the confiscation, the full suppression of the dream image. Mental confiscation is usually called "forgetting." One relates with wonder how clearly he dreamed, and yet when he woke all was confused and in a few minutes he had forgotten it all. At other times one can only say that the dream was beautiful, good, bad, confused, stimulating, or stupid. Even in making this judgment many times a remnant of the dream content will show itself, whose analysis can lead to a later recovery of larger fragments of the dream. Behind such additional fragments of the dream brought to light in this way one often finds the kernel of the dream-thoughts.

It is an important consequence of Freud's theory of dreams, that one is always dreaming, so long as he sleeps. That one

remembers nothing of this is no decisive objection against this consideration. My patients, for example, who at the beginning of the analysis declared that they had usually no dreams, gradually accustomed themselves by continual weakening of the inner psychic resistance against the censorship to remember all their dreams. But if in the course of the analysis one strikes a very resistant, pain-toned complex, dreams apparently cease—naturally they are only forgotten, repressed, because of their unpleasant content.

The obvious objection, that these dream observations and analyses have been carried on for the most part on neurotic and so abnormal persons, and that conclusions should not be drawn as to the dreams of healthy people, does not need to be refuted by the reply that mental health and psychoneuroses only differ in a quantitative way; it can also be answered that the analyses of people mentally normal fully agree with the interpretation of dreams of neurotics. The communication of the analysis of one's own dreams, however, meets with almost insurmountable difficulties. Freud has not shrunk from this sacrifice—the exposure of intimate personal matters—in his "*Traumdeutung*," even though regard for others make unavoidable gaps here and there in his analyses. Similar considerations made it necessary for me to explain the art of the interpretation of dreams not from my own dreams, but from those of my patients. For the rest, the practise of self-analysis is indispensable for any one who desires to penetrate into the unconscious processes of dream-life.

The neurotic persons whose dreams I have brought forward here and there as examples also pave the way for me to say a few words about the pathological and therapeutic significance of dreams and their interpretation. We saw how greatly the analysis of a neurotic may be accelerated by a successful dream analysis. The dream censorship, which is only half awake, often allows to penetrate to the dream consciousness thought-complexes, which in waking life could not be brought to consciousness by free association. There also lead out from the dream elements immediate and shorter ways to the repressed pathogenic material, that is, to the source of the neurotic symptoms. The becoming conscious of such complexes can be regarded as a step toward the cure.

Then, too, the diagnostic significance of dreams must not escape us, and in a time which is not too distant there ought to arise besides the physiological, also a pathological dream psychology, which should treat systematically of the peculiarities of dreams with hystericals, those suffering from imperative neuroses, paranoiacs, dementia præcox patients, sufferers from neurasthenia, from the anxiety-neurosis, alcoholism,

epilepsy, paralysis, sub-normals, etc. Many pathognostic peculiarities of dreams in these diseased conditions are already recognizable to-day.

All these more practical and special questions were raised to importance by the unexpected theoretical consequences of this investigation of dreams. Freud has succeeded in surprising a process on the boundary line between the physiological and pathological departments of mental life, in taking it in the midst of its work, *in flagranti*, as it were. In this way he has brought us nearer to an understanding of the mechanism of the manifestations of neuroses and insanity in waking life. And though it was the study of psychoneuroses which led Freud to his investigation of dreams, the dream theory pays back with interest all that it owes to pathology.

The case could indeed not be other than it is. Waking, dreams, neuroses and psychoses are only variations of the same psychic material with different modes of functioning, and progressive insight into one of these processes must necessarily deepen and widen our knowledge of the others.

Those who expect from the new dream theory any sort of prophetic insight into the future will turn back disillusioned. But those who value highly the solution of psychological problems held until now as insoluble, the widening of the psychological point of view apart from any immediate practical consequences, and who are not held back from advance by hide-bound prejudices, — such will perhaps supplement the communication given here by a thorough and earnest study of Freud's "*Traumdeutung*."¹

¹ Prof. S. Freud: *Die Traumdeutung*, 2d ed., Deuticke, Vienna, 1909.

PSYCHOLOGICAL LITERATURE

Mendel's Principles of Heredity, by W. BATESON. Cambridge, The University Press; New York, G. P. Putnam's Sons, 1909. pp. xiv, 396. Price \$3.50 net.

In 1902 Professor Bateson published, under the title *Mendel's Principles of Heredity: a Defence*, a translation of Mendel's paper on Hybridization, together with a brief summary of Mendelism as then developed. The book quickly went out of print, and was not republished. The object of the present work, which in a sense takes the place of its predecessor, is to give a succinct account of discoveries in regard to heredity made by the application of Mendel's method of research. Theoretical considerations, the bearing of the new facts on the great problems of biology, are here reduced to the briefest possible indication; they will be dealt with separately in another book, based on the author's Silliman Lectures of 1907.

We have before us, then, a sort of text-book of Mendelism, in so far as the doctrine is capable of representation in text-book form, written by one of its foremost expositors. Ch. i, Introductory: Mendel's Discovery, gives a brief historical outline, and thereafter illustrates and defines various technical terms: dominant and recessive, segregation and allomorphism, homozygote and heterozygote, purity of type. "The fact of *segregation* was the essential discovery which Mendel made. . . . The *dominance* of certain characters is often an important but never an essential feature of Mendelian heredity. . . . Purity of type is dependent on gametic segregation, and has nothing to do with a prolonged course of selection, natural or artificial." The account given of these fundamental matters is condensed, and needs careful reading, but is sufficiently clear to the advanced student.

Ch. ii, The Material Investigated, begins with a very useful selected list of structural characters in plants (36) and animals (25) whose inheritance follows the general rules described in the preceding chapter. References to the original memoirs, and brief summaries, are appended in every case. The author then turns to the subject of color, which is presently to occupy him through several chapters, and enumerates the animals and plants in which color characters have been shown to have a Mendelian inheritance. In conclusion, he touches on various general questions. No class of characters has as yet been identified to which the Mendelian system is demonstrably inapplicable, though the future cannot, of course, be foretold. One meristic character (brachydactyly in man) is Mendelian; the study of such characters is now of especial importance. Mendelian principles have been proved to apply to wild types, and are thus not confined to unions of pure races. There is no distinction between inter-racial and intra-racial heredity. Dominance is not necessarily an attribute of the phylogenetically older character. A dominant character is the condition due to the presence of a definite factor; the corresponding recessive owes its condition to the absence of the same factor. The chapter ends with a statement of the salient differences between the Mendelian and the Galtonian theories of inheritance. The statement is admirably clear; and, indeed, the sharp division drawn between Mendelism on the one hand and biometry on the other is one of the best features of the book.

Ch. iii, Numerical Consequences and Recombinations, deals illustratively (combs of fowls, heterostylism in *Primula*) with the proportions of the F_2 generation and with novel types produced by recombinations.

The succeeding five chapters discuss in detail the problem of heredity of color. "Taking the evidence respecting the genetics of color as a whole, . . . there can be no reasonable doubt that with rare exceptions it will be found possible to express the whole series of phenomena as due to the combination and recombination of a limited number of recognizable factors, which are treated by the cell-divisions of gametogenesis as units. . . . One positive deduction cannot be overlooked: that the organism is so built that definite additions to, or subtractions from, its totality may readily be made by Variation, and that the consequence of such alteration of the ingredients may be recognizably definite or, to use another term, specific."

Ch. ix deals with gametic coupling and spurious allelomorphism. Certain phenomena "indicate a system of segregation taking place in such a way that gametes presenting certain . . . combinations occur with greater frequency than the others." Instances are drawn from the pollen-shape, contabescence of anthers, and color of the Sweet Pea. Spurious allelomorphism occurs when factors concerned with features of organization which seem to have no special physiological association behave as allelomorphic to each other. "Two dominant or 'present' factors behave as if in the cell-divisions of gametogenesis they repelled each other. . . . The dividing cell being $AaBb$, the daughter-cells are respectively Ab and aB ." The author recognizes the possibility of disturbance by selective attraction between different kinds of gametes (selective mating), though much more evidence is required for anything like demonstration.

Ch. x reviews the facts relating to Heredity and Sex, with strict limitation, of course, to Mendelian experiments. The main outcome is that in certain forms the female is a sex-heterozygote, with femaleness dominant; the female is a hybrid, female-male, while the male is pure male, or male-male. Since, on the other hand, the cytologists show that in most orders of Insects proof that the male is heterozygous can be obtained, the author concludes: "Improbable as at first sight it may appear, the view that most commends itself to me is that in different types Sex may be differently constituted." The fact that the females of a true-breeding strain may be hybrid in some important respect, while the males are not, brings us a step nearer the discovery of the nature of Variation.

Ch. xi, on Double Flowers, we may pass over. Ch. xii, on Evidence as to Mendelian Inheritance in Man, brings together such observations of inherited traits, normal and abnormal, as can be found; the evidence, however, is scanty, since man has for some reason or other—hardly, one would think, for the reason alleged by the writer, the "special difficulties attending the study of human heredity;" since these are as great for Galtonians as for Mendelians—been left almost entirely to the biometrists. Eye-color, brachydactyly, cataract, tylosis, etc., are discussed in sufficient detail, and the chapter ends with a note on collecting evidence as to human descent: a note which gains in interest by the insertion of a slip, on which Professor Bateson confesses that his representation of the inheritance of color-blindness "contains a serious error." The reviewer may take occasion to say that, not only here but throughout the work, the author makes an impression of the utmost candor and frankness; mistakes and ignorance are signalized, no less than success and successful prediction (*cf.* p. 209, the mulatto; p. 128, the Basset hound).

Ch. xiii, on Intermediates between Varieties and the Pure Lines of Johanssen, is of crucial importance. Intermediate and gradational forms undoubtedly appear; how can Mendelism cope with them? Professor Bateson grants that analysis is as yet incomplete and must be laborious; but he urges that in many cases the intermediate character is probably only a superficial or net result of the interaction of factors which are transmitted as units. Sometimes, *e. g.*, the whole group of heterozygotes forms a recognizable class which may be described as intermediate between the two pure types (blue Andalusian fowl). Or intermediates may be due to subtraction-stages of dominant factors (color of the Dutch rabbit; half-dwarfness in peas), or to the interference of other factors (English pattern of rabbit; Painted Lady form of Sweet Pea). And finally there are intermediates due to the disturbing effects of many small causes not of genetic but presumably of environmental origin, fluctuational forms whose intermediacy is not transmissible. In view of all these possibilities, it is evidently incautious to assert that in any specific case segregation does not occur.

Ch. xiv takes up certain Miscellaneous Exceptional and Unconformable Phenomena, to wit, cases in which crosses breed true without segregation, departures from numerical expectation, irregularities of dominance, alternation of generations, maternal characters in embryos. In most instances, the author is able to suggest at any rate a possible and plausible explanation of the anomaly. Alternation of generations is, however, as he confesses, a phenomenon which at present is incapable of factorial representation.

Ch. xv briefly considers Biological Conceptions in the Light of Mendelian Discoveries. "Much that is known of chromosomes seems inconsistent with the view that they are the sole effective instruments in heredity." Variation must be regarded in the main as a phenomenon due to the addition or omission of one or more definite elements. Reversion occurs when the sum-total of the factors returns to that which it has been in some original type; reversion on crossing is thus merely the special case in which one or more missing factors are brought in by the parents of the cross-breed. As for the bearing of Mendelism on the theory of evolution, the following may be said. (1) In countless instances segregation plays a part in the constitution and maintenance of characteristics held by systematists to be diagnostic of species. De Vries' distinction of specific and varietal (non-segregating and segregating) characters cannot be accepted. (2) There is a real difference between fluctuating variations and actual genetic variations. By the latter alone can permanent evolutionary change of type be effected; and they are commonly, though not always, sufficiently discontinuous to merit the name Mutations. (3) There is nothing in Mendelism that runs counter to the doctrine of Natural Selection, although the scope of that principle is closely limited by the laws of variation.

The concluding chapter xvi, on the Practical Application of Mendelian Principles, should be read in connection with the Preface, which extends the discussion of what the fancier or breeder has to expect from Mendelism. Mendelian discovery, as we have seen, abolishes the old idea that time and continued selection are needed in order to make a variety breed true. Certain types are unfixable, for the simple reason that their special character is a special consequence of the meeting of dissimilar gametes. Sociologically, Mendelism suggests a mode of procedure the opposite of that favored by current eugenics; certain serious physical and mental defects, almost certainly also some morbid diatheses, and some of the forms of vice and criminality

could be eradicated if society so determined; but any attempt to distinguish certain strains as superior, and to give special encouragement to them, would probably fail to accomplish the object proposed, and must certainly be unsafe. The author adds that "society has never shown itself averse to adopt measures of the most stringent and even brutal kind for the control of those whom it regards as its enemies."

The book concludes with a biographical notice of Mendel, three portraits of whom are inserted, and with translations of Mendel's papers on Hybridization and on *Hieracium*. There are six colored plates, and a number of figures in the text. The mechanical side of the work is worthy of its spirit and contents. For though a new edition will be called for every few years, as facts accumulate and theories are revised, there can be little doubt that the *Principles of Heredity* will take rank as a classical exposition of its subject from the Mendelian standpoint.

L. TURLEY.

American Primitive Music, with especial attention to the Songs of the Ojibways. By FREDERICK R. BURTON. New York, Moffat, Yard & Co., 1909. pp. v, 281+73+7. Price, \$5.00 net.

Mr. Burton, who is a composer of recognized merit and has served as musical expert in the ethnological departments of the American Museum of Natural History and the Field Columbian Museum, has written this book rather as musician than as ethnologist. "That Indian songs may be useful to civilization, that is, that they have great art value, I thoroughly believe, and I should be lacking in the courage of my convictions if I did not make such demonstration of my belief as lies in my power." Nevertheless, he realizes that the acoustical side of primitive music cannot be ignored even by one who applies himself mainly to the æsthetic, and accordingly does not scruple to express his dissent from the conclusions of certain ethnological enquirers who have previously written on the subject of Indian music.

To illustrate the artistic value of the Ojibway song, the author has selected twenty-eight numbers from his collection, has adapted to them English verse suggested by the Indian originals, and has provided them with pianoforte accompaniment; some of the songs he has also arranged for unaccompanied mixed quartette. Opinions will doubtless differ, both as to the intrinsic value of the themes and as to the possibility of any widespread infusion of Indian ingredients into our own music: the reviewer must acknowledge that, in his judgment, many of these songs have both charm and virility.

Mr. Burton has, further, given the notation of his whole collection of nearly one hundred songs as recorded by the phonograph, together with the Indian words (so far as intelligible) and their English translation. The notation raises, of course, the whole question of scale. The writer ascribes to the Ojibways two pentatonic scales, major (*sol, mi, re, do, la, sol*) and minor (*mi, re, do, la, sol, mi*); each of these is developed by the addition of one tone which brings about a scale relationship closely analogous to the ancient hexachord; major, *sol, mi, re, do, si, la, sol*, and minor, *mi, re, do, si, la, sol, mi*. There are also certain songs that appear to be based upon the diatonic major scale of civilization. How far all these things are original, and how far their finish and perfection are due to civilized influence, Mr. Burton does not attempt to say; it is enough for him to appreciate the primitive character of the music as a whole. He has, however, in his remarks upon Mr. Gilman's examination of the Hopi songs, an argument that is suggestive, and may be outlined here. Choruses, he says, composed of persons who know the scale

and are familiar with modulation, often flat in course of rehearsal to a semitone or more below the initial pitch; solo singers are frequently off pitch. Now suppose that we had no system of notation, but were limited to phonograph records of actual performance; and suppose that these records were examined by help of a specially tuned harmonical, and the exact pitch of every tone noted. Would not the examiner be justified in attributing to us the conception of adiatonic intervals?

Mention must be made, finally, of the chapter on Rhythm, in which the author corrects the exaggerated views of certain previous writers. It is not true, he declares, that primitive man has developed rhythm to a plane higher than that attained by civilization; neither is it true that his conception of rhythm is wholly at variance with ours. The fact is that, to the Indian, the drum is primary. The dance is the vehicle of the expression of his deepest feelings; dance and song almost always go together; to drum is instinctively with him to set the tempo and mark the rhythm for a dance. Habitually and irresistibly he drums with steadiness, according to a set plan, varying the stroke only when some uncommon feature of the dance calls for a change of step or tempo. But now comes the development of melody; there is conflict between voice and drum, and the voice weakens. "Melody, therefore, became distorted; it was hindered in its natural development, struggling always to assert its spontaneous freedom, and always restrained by the habit of the drum, which the Indian would abandon no more readily than he would abandon any other of his numerous traditions. . . . Both, drumbeat and song, are ingenuous expressions of his nature. One is extremely primitive, the other comparatively advanced, and as he is still primitive he clings to his cheerful noise, understanding it, aroused by it, while his musical soul toils darkly on toward an expression that aims ever at and sometimes attains symmetry. All of which is to say that he drums as he does because he knows no better."

Difficulty arises, then, only when the attempt is made to square up the time-value of the notes sung with that of the drum-rhythm. A singer "will start his drum in 9-8, for example, and begin bravely to sing against it in 4-4; but after a few measures of success he breaks away, and from then on the value of his notes can be expressed only approximately." The drum may be as steady as a metronome; but if the series of beats is plotted out in relation to the voice, a visual illusion of irregularity of rhythm must necessarily be produced. This view is, without any question, to be preferred to the rival theory.

EDWARD P. HAVELOCK.

Beasts and Men: being Carl Hagenbeck's Experiences for Half a Century among Wild Animals. An abridged translation by H. S. R. Elliot and A. G. Thacker. With an Introduction by Chalmers Mitchell. Photogravure portrait of the author and 99 other illustrations. London and New York, Longmans, Green & Co., 1909. xiii, 299.

This is one of the most interesting, as well as one of the best appointed books upon popular science that appeared during the winter holiday season of 1909. As the title indicates, it gives Carl Hagenbeck's own account of his life and fortunes, from the first modest establishment in the Spielbudenplatz at Hamburg to the creation of the great Zoölogical Park at Stellingen. Methods of capture, methods of transportation, of housing, of feeding, of training animals are set forth in an admirably direct and simple manner; and the narrative is interspersed with anecdotes of exciting events in the career of Mr. Hagenbeck himself or of his travellers and associates.

The book is essentially popular in intention; and Mr. Hagenbeck is a trainer and exhibitor of animals, not a professional naturalist. Nevertheless, his pages are full of accurate and detailed statements which cannot but be of service to the student of comparative psychology, who is concerned to maintain his subjects in full health and under conditions as nearly natural as possible. He insists, for instance, as Darwin had insisted before him, that animals have their special temperaments, their idiosyncrasies. "It is now universally recognized that each animal has its own peculiar characteristics . . . over and above the general psychological character which it shares with all other members of its species. This is a discovery I had to make for myself, and a most important one it is for the trainer. . . . On the occasion of my first attempt to introduce the humane system of training, out of twenty-one lions only four proved to be of any use for my purpose." The fact has, of course, been amply verified by recent experimental work upon the higher animals. As regards the humane method of training, Mr. Hagenbeck is enthusiastic; Dr. Mitchell, in his prefatory note, while he freely admits the author's own love of animals, and his ability and experience in dealing with them, confesses to a continued scepticism. The humane method appears to be a method of infinite patience, sanctioned by moderate reward and moderate punishment; it is evidently, therefore, a method only for the elect among trainers. The psychologist must regret that he is not taken further behind the scenes; but the topic would probably fail to interest the general reader.

Another point of great interest is this: that even exotic animals may be acclimatized, if only they are allowed air and exercise. Photographs are shown of ostriches, Dorcas gazelles, lions and kangaroos ranging freely in the snow at Stellingen. Mr. Hagenbeck's experience here confirms and extends that of the famous Crimean naturalist, M. Falz-Fein, of the Duke of Bedford and of Lord Rothschild. In view of the approaching extermination of much of the African and Australian fauna, the author suggests the formation of a large park in Florida; a reserve of even 1,000 acres would do good zoölogical service; and the initial cost need not exceed \$250,000. The excellence of the climate would render unnecessary most if not all of the usual expense of special, massively constructed houses with elaborate heating-arrangements, etc. Indeed, on the open-air system, this expense is in the main avoidable even for the ordinary town-gardens; and Mr. Hagenbeck thinks that there is no town of 100,000 inhabitants that may not have its collection of animals, administered at trifling cost and with small risk of loss.

The chapters of the book are entitled: *My Life in the Animal Trade*, *My Park at Stellingen*, *How Wild Animals are Caught*, *Carnivores in Captivity*, *Training Wild Animals*, *The Great Herbivores*, *Reptiles in Captivity*, *Acclimatization and Breeding*, *Animals in Sickness*, *Life at Stellingen*, *The Ostrich Farm at Stellingen*, and *Anthropoid Apes*. All are freely illustrated from photographs. In the concluding chapter, to which the psychologist naturally turns with especial interest, the trainer practically excludes the naturalist in Mr. Hagenbeck's account, though there are a few observations of scientific interest. "I am hoping before long," the author remarks, "to be able to exhibit such educational results in my apes as have never been achieved or even thought possible before."

FRANCIS JONES.

The Origin and Development of the Moral Ideas, by EDWARD WESTERMARCK. London, Macmillan and Company, Vol. i, 1906. pp. xxi, 716. Price, \$3.50.

This work, by the illustrious author of *The History of Human*

Marriage, exemplifies a tendency of deep import in current ethical writings; the tendency, namely, to study moral concepts in the light of history and ethnology, and to regard moral acts and judgments from the standpoint of the science of mind. The work arose from the queries: 'Whence the agreement and difference among moral ideas?' and 'Why moral ideas at all?' Westermarck has sought the answer, not in a *a priori* argument, but in a first-hand analysis of the moral consciousness as it comes to expression in human conduct at large.

Moral judgments rest upon 'moral' emotions, and moral concepts refer to the tendency in certain phenomena to elicit these emotions. The establishment of morals upon an emotive basis calls, of course, for a differential description of the moral consciousness. This is a difficult task. The author enters upon it as follows: Moral emotions are either of approval or disapproval. Both are species of 'retribution,' which includes also non-moral forms of 'resentment' and 'gratitude.' The common element in all resentment (*i. e.*, moral and non-moral disapproval) is "an aggressive attitude of mind towards an assumed cause of pain," and in all 'kindly retributive' emotion, "a friendly attitude of mind towards a cause of pleasure." The *moral* forms, now, are marked by (1) disinterestedness, (2) impartiality, and (3) generality. Both for the survival of moral emotions and for their origin, Westermarck has a teleological explanation. Hostility toward the cause of pain and retributive friendliness toward the cause of pleasure are useful; they tend "to promote the interests of those who feel them." The origin of the distinctively moral qualities of emotion is social; the emotion is disinterested, impartial, and generalized, because the situation is public. That is, it concerns custom; it is tribal; and it reflects a social rule of conduct.

As an account of the origin of morality, the argument appears to move in a circle. "As the rule of custom is a moral rule, the indignation aroused by its transgression is naturally a moral emotion;" but "custom is a moral rule only on account of the indignation called forth by its transgression." At most, the argument proves that morality is very old, and that its essential characteristics are discoverable in primitive tribal customs. Moreover, in the moral consciousness the primacy of the emotion (as against the 'judgment') is only partially established; for it is to be noticed that all the qualities that are properly 'moral' (see above) are not, in fact, emotive in their nature. Really, in order to make his point, Westermarck should have gone deeper and given an analytic description of the observer's consciousness, instead of giving—as he actually does—the observer's 'moral' *opinion* of an act or situation. Possibly this is asking too much in the present condition of the psychology of the emotions. If the analysis had been made, however, the author might conceivably have discovered that pleasure and pain are not the *only* objects of approval and resentment, and, further, that the moral consciousness is as much conative as emotive.

His method in the study of moral concepts is more effective. It endeavors "to fix the true import of each concept by examining how, and under what circumstances, the term expressing it is generally applied," and it tends to show that the concepts "are all fundamentally derived from either moral indignation or moral approval." With Chapter vii begins the most valuable part of the work; the scrutiny, namely, of the "mass of phenomena which, among different peoples and in different ages, have had a tendency to call forth moral blame and moral praise." The sources are customs and laws. The chapters which discuss the relation of custom and law to ethics, and those

which treat of motive, will, and conduct as subjects of moral approbation and resentment, are among the finest in the book. The last half of this volume is devoted to the first of six different modes of conduct regarded in the concrete, *i. e.*, that mode of behavior which affects the welfare of other men. It is here particularly that Westermarck's acquaintance with classical and ethnological sources is most skillfully and effectively used. Here is portrayed the actual moral and immoral life of mankind set in its natural environment of social conduct. Westermarck's method and material are alike destined to exert a profound influence upon the science of ethics. The reproach of 'objectivity' is certain to be brought; the criticism that the moral consciousness is made to dwell too exclusively upon the ethical value of the acts of others, to the disregard of the subject's own good or bad will. But this reproach is to be met, in the reviewer's opinion, rather by an effective system of moral prophylaxy and moral hygiene than by the introduction of a subjective attitude into the scientific study of the moral life.

MADISON BENTLEY.

Cornell University.

Charles Darwin and the Origin of Species: Addresses, etc., in America and England in the Year of the Two Anniversaries. By E. B. POULTON. Longmans, Green & Co., London, 1909. pp. xvi, 302.

In this volume, published Nov. 24, 1909, on the fiftieth anniversary of the publication of the *Origin of Species*, Professor Poulton has brought together the various essays and addresses which he prepared for the Darwin celebrations in England and the United States. His general standpoint is defined in the Preface as follows: "The Darwinian of the present day holds an intermediate position between the followers of Buffon and Lamarck and the Mutationists . . . The disciple of the two first-named naturalists, in these days calling himself an ecologist, maintains that organisms are the product of their environment; the Mutationist holds that organisms are subject to inborn transformation, and that environment selects the fittest from among a crowd of finished products. The Darwinian believes that the finished product or species is gradually built up by the environmental selection of minute increments, holding that, among inborn variations of all degrees of magnitude, the small and not the large become the steps by which evolution proceeds." This, then, is the point of view of the book. Ch. i, Fifty Years of Darwinism, reprints, with some important changes, the essay which gave its title to the volume of Centennial Addresses reviewed in the *Journal*, xx, 1909, 578 ff. Ch. ii touches lightly but appreciatively on the Personality of Charles Darwin. Ch. iii, on the Darwin Centenary at Oxford, discusses the reasons for Darwin's self-confessed loss of the faculty of æsthetic enjoyment; the writer seems to have missed Titchener's paper on the same subject in the *Pop. Sci. Mo.* Ch. iv rehearses Darwin's relation and debt to the University of Cambridge. Ch. v, The Value of Color in the Struggle for Life, is a somewhat extended reprint of the author's contribution to the English memorial volume, *Darwin and Modern Science*. Ch. vi, Mimicry in the Butterflies of North America, shows by reference to special cases that the study of mimicry possesses great advantages for an understanding of the history and causes of evolution, and incidentally outlines a number of problems for American investigators.

Ch. vii breaks new ground; it contains a series of letters written by Darwin to Mr. Roland Trimen between the years 1863 and 1871. The letters belong to an interesting period and, as the editor remarks, "show all the characteristics of Darwin, in his relations with younger men who helped him in his work."

The volume ends with four Appendices. In the first of these, Professor Poulton collects Darwin's arguments against the hypothesis of multiple origins of species. In the second, he brings together, in like manner, Darwin's utterances on evolution by mutation. In the third he returns to the æsthetic question, and proves that scientific work was necessary to Darwin's physical well-being. In the fourth he unearths a divergence of opinion, as between de Vries and certain of his followers, on the subject of the hereditary transmission of fluctuating variations. But surely the divergence is apparent only; the author has failed to distinguish between minute variation that is ancestrally determined and the fluctuation exhibited by pure lines.

TH. WALTERS.

The Family and the Nation, a Study in Natural Inheritance and Social Responsibility. By W. C. D. and C. D. WHETHAM. Longmans, Green & Co., London, 1909. pp. viii, 233.

This interesting and well-written essay is a reasoned plea for a practical eugenics. Civilization is in danger from the lessening of the action of natural selection; of late years, the means of keeping alive the falling and the fallen have grown with ever-increasing speed; and humanitarianism has tended towards sentimentality. At the same time, the social organism has grown self-conscious; there is a new-won appreciation of the issues at stake. Hence it is in order to inquire how far selection, natural and artificial, has been the means of developing the race, how far it is still acting and in what directions, what will be the effect of that action, and whether it can be controlled in any way to favor the preponderance of the best physical, mental and moral qualities.

If selection is to work, individuals must vary, variation must be inherited, and certain kinds of inherited variation must reproduce themselves at a quicker than average rate. We shall, therefore, in the pursuit of the inquiry outlined above, begin with the consideration of the laws of variation and of heredity. The authors give, first, a general discussion of the scientific study of these topics, illustrating them by reference to simple cases of Mendelian inheritance, of normal distribution, etc.; incidentally they show that Galton's law of ancestral inheritance may be reconciled with the Mendelian principle of particulate inheritance if, instead of a single individual, we consider large numbers: "the frequency of Mendelian dominance would produce, on the average of large numbers, greater resemblances of children to their parents than to their grandparents and to more distant ancestors." They then treat, chiefly on the basis of Galton's work, of inheritance and variation in mankind. Special chapters are devoted to the inheritance of mental defect and ability, and to the rise and decline of families. At this point the authors turn to the third condition of the operation of selection, the necessity of reproduction, and discuss in three chapters the birth-rate, the selective birth-rate: its effects, and the decline in the birth-rate: its causes. "In the British Isles certainly, and probably in Western Europe generally, the best elements of the population are increasing, if they increase at all, at a much slower rate than the less worthy stocks, and, in some cases at any rate, the better classes are actually diminishing in number." The outcome must be deterioration, and eventually the passing of the race. Why, then, do the worthier classes desire to restrict their offspring? The authors find a number of contributory causes: the feeling of overwhelming responsibility towards children, expense, the advent in society of persons whose newly acquired wealth is not associated with definite territorial or local traditions, the cult of games, the restless-

ness and uncertainty of modern life in various professions, the freedom of women, etc. Fortunately, the reserve of health, strength and ability in the people is still very great; and, fortunately, the tone of public opinion may be changed by the influence and example of those who are awake to the danger. Mr. and Mrs. Whetham accordingly end their work, in a fairly hopeful spirit, with the following appeal: "Encourage in all ways early marriages and large families for men and women of health, strength and ability; discourage both marriage and offspring where either parental stock is unsound in body or mind." The advice is admirably sound, and the hopefulness, in all probability, is not misplaced. For there is nothing that so much strikes the outside observer of recent public opinion in England as the steady progress made, in spite of prudishness and conservatism, by the new creed of eugenics. S. POST.

The Autobiography, a Critical and Comparative Study, by A. R. BURR.
Boston & New York, Houghton Mifflin Co., 1909. pp. viii, 451.
Price, \$2.00 net.

In this *Journal*, xix, 415, I called attention to the first volume of Professor Misch's *Geschichte der Autobiographie*, a work planned and in part executed with the traditional German thoroughness. Mrs. Burr has, in the volume now before us, treated of the autobiography in lighter vein. Writing on the basis of "two hundred and sixty capital autobiographies," she has produced a very interesting book, literary in flavor, psychological in suggestion, which should do much to arouse her readers to further and more intensive study of a fascinating subject.

Mrs. Burr may be quoted, in large measure, as her own reviewer. "The indication is plain," she writes, "that a subjective trend of thought made its appearance in literature, rather suddenly than slowly, during the first three hundred years of the Christian era. Examination of its early manifestations shows the primal cause to be religious emotion: for the second type of the subjective document—the scientific—did not make its appearance until the sixteenth century [ch. iii, History]. When one turns to the documents themselves, an investigation begins most naturally with a comparison of the reasons for writing them, and of the attitudes they take, with like attitudes in diaries and in letters. . . . Works written according to the autobiographical intention are written 'as if no one in the world were to read them, yet with the purpose of being read' [ch. ii, Classification and the Autobiographical Intention; ch. v, The Autobiography, the Diary, and the Letter]. Conformation to this standard permits us (always within recognized limits) to believe in their sincerity and to trust their information" [ch. iv, Sincerity; ch. ix, the Autobiography in its relation to Fiction]. Julius Cæsar, St. Augustine and Girolamo Cardano are considered as the three great archetypes of autobiography, and the latter's *De vita propria liber* receives a chapter to itself [chs. vi, vii]. After tracing the influence of these models upon later times [ch. viii], the author reaches a formulation of the law of the subjective self-study, which is "that its manifestations invariably precede and accompany movements of intellectual significance; and that, conversely, in times when great warlike activities and political upheavals make their special demand upon the objective energies of a people, the subjective record diminishes in proportion, or wholly disappears from literature" [ch. x]. This same chapter [The Autobiographical Group] also "attempts to give some conception of the part these documents may be permitted to play in sociological and historical investigation."

So far the first and general part of the book. The second and special

part takes up the works selected under various partial and especially suggestive aspects: religion, humor, self-esteem, genius, sex relations, and so forth. Here the author reaches certain conclusions of interest, as that the relation of the sexes to each other has changed less with the centuries than attitudes toward nature or toward society at large; that the prevailing happiness of the intellectual life negates the pathological theories of genius; that the study of religious confession has hitherto been illusory and misleading. The work here is admittedly selective and incomplete; but, again, the reader is pleasantly directed to the original sources. The volume closes with a series of bibliographical appendices.

F. E. BARBOUR.

Journals of Ralph Waldo Emerson. With Annotations. Edited by Edward Waldo Emerson and Waldo Emerson Forbes. Boston and New York, Houghton Mifflin Co. Vol. i, 1820-1824. 1909. Vol. ii, 1824-1832. 1909. pp. xxvii, 394; xvii, 542. Price per vol. \$1.75 net.

As a supplement to the centenary edition of Emerson's works, the publishers have decided to issue an edition of his journals. The first intention was to begin the publication with the year 1833, after Emerson's return from his first visit to Europe; "but, on carefully reading the journals for the fourteen years preceding that time,—for the boy faithfully kept them from the age of seventeen onwards,—it seemed well to the editors to introduce large extracts from these." "The extracts from the early journals are not chosen for their merit alone: they show the soil out of which Emerson grew, the atmosphere around, his habits and mental food, his doubts, his steady, earnest purpose, and the things he outgrew. His frankness with himself is seen, and how he granted the floor to the adversary for a fair hearing." These first volumes cover Emerson's life at college (beginning Feb., 1820; ending June, 1821); his experiences as school teacher and divinity student (1822-1826); the period elapsing between ordination ('approbation to preach') and engagement (end of 1826 to end of 1828); and the ministry of the Second Church of Boston (1829-1832). The Editors have performed their task admirably; and the books themselves, illustrated by photogravure and other plates, and well printed upon a light cream colored paper, do credit to the publishers.

M. W. WISEMAN.

The Economy and Training of Memory. By H. J. WATT. New York, Longmans, Green & Co., 1909. pp. viii, 127.

The author of this little book received his experimental training in Külpe's laboratory at Würzburg, and is favorably known by his doctorate thesis ('Experimental Contributions to a Theory of Thought') and by later publications in Meumann's *Archiv*. He has here attempted to make accessible to students and teachers the most valuable of the experimental results obtained during the past quarter century in the study of memory and related subjects. After a general introduction, he discusses the experimental investigation of memory, some general questions (child and adult, memory and intelligence, etc.), the factors which influence memory, mental imagery, thoughts, and rules for the economy and training of memory. The work is simply written, and the conclusions and recommendations are in general sound. If criticism has anything to remark, it is that the author betrays an occasional tendency to generalize and deduce beyond the limits of his experimental data. In this respect, his book is inferior to that of Offner (*Das Gedächtnis*: see this *Journal*, xx, 1909, 457). J. FIRE.

American Education. By ANDREW S. DRAPER, Commissioner of Education of the State of New York. With an Introduction by Nicholas Murray Butler, President of Columbia University. Boston, New York and Chicago, Houghton Mifflin Co., 1909. pp. x, 383.

This volume contains twenty-six papers upon educational topics, selected from the much larger collection of Commissioner Draper's essays and addresses. Eight of these deal with the general topics of organization and administration; five with the elementary and secondary schools; six with the college and the university (the American university, the trend in American education, state universities, the university presidency, limits of academic freedom, co-education); and the remaining seven with special aspects and problems (education for efficiency, the farm and the school, physical training and athletics, public morals and public schools, the spirit of the teacher, the teacher and the position, the schools and international peace). "No other American," writes President Butler, "has been successively charged with the administration of a state system of public instruction, with the oversight of the schools of a city of considerable size, with the direction of one of the tax-supported state universities of the country, and finally with the supervision and control of the educational activities of an entire commonwealth. As a result, Mr. Draper has been forced, in the daily performance of the duties of his several offices, to approach the educational problem from many different points of view and to see it under almost all of its limitations and difficulties." Mr. Draper has, indeed, enjoyed an unrivalled experience; and he has turned this experience to account in a direct and forcible way; his writing is clear, explicit and concise. Unfortunately, the many different points of view that he has taken do not, in the judgment of the reviewer, include that of the true educator. Mr. Draper's standard is consistently that of the man of affairs; an educational system is, for him, a plant, with a foreman and a staff of employees; and the end and aim of education is efficiency. By many his book will be hailed as the very gospel of a new educational dispensation; but some will regret that a man who fills and has filled such responsible offices should be so narrowly commercial in his outlook.

FRANCIS JONES.

Mental Discipline and Educational Values, by W. H. HECK. New York, John Lane Co., 1909. pp. 147. Price, \$1.00 net.

This essay has been written with a twofold object: first, to sum up and organize recent discussions of the disciplinary value of studies, in order to show how far students of education have advanced in their thought upon the subject; secondly and more importantly, to modify the doctrine of formal discipline and upon such modification to establish a standard of educational values. The writer accordingly presents, in the first part of the book, a long series of carefully chosen extracts from educational authorities; the utility of this portion of his work would have been increased by the addition of an index. On the main issue, Professor Heck concludes as follows: "A general benefit can be derived from specific training in so far as the person trained has consciously wrought out, in connection with the specific training, a general concept of method, based upon the specific methods used in that training."

OTTO PERLER.

The Sunday Kindergarten; Game, Gift and Story. By C. S. FERRIS. Chicago, University of Chicago Press, 1909. pp. xxvi, 271. Price, \$1.40.

This book has been issued as an aid to the religious education of

very young children. It contains forty-three lessons upon the topics of dependence, kindness, generosity, love, courage, obedience, immortality, helpfulness; every lesson is based upon a story, which may or may not be taken from the Bible; and the programme is arranged to carry the teacher through the Sundays from autumn to summer, with appropriate lessons for Thanksgiving, Christmas and Easter. A regular order of exercises is outlined, and the words and music are given of twenty-one selected songs. The book is well illustrated, and in general has been carefully prepared; it should prove excellently adapted to its purpose.

F. SMITH.

The Child and His Religion. By G. E. DAWSON. Chicago, University of Chicago Press, 1909. pp. ix, 124. Price, 82c.

The core of this little book is the chapter entitled *Children's Interest in the Bible*, which is reprinted without substantial change from the *Pedagogical Seminary*, 1900. Two prefatory chapters deal respectively with Interest as a Measure of Values and with the Natural Religion of Children. In the former, the author sketches the doctrine of interest in its various historical phases, and enters a plea for its acceptance in religious as in secular education. In the latter, he seeks to determine the psychological factors in natural religion, and finds them in animism, the instinct of causality, the instinct of immortality, and the child's inherent faith and good-will. A concluding chapter outlines the problem of religious education. Its aim is that of religious adjustment to a progressive environment; its material is the whole of experience, religious in the stricter sense and secular as well, appropriated to religious uses; and its method is that which insures self-expression, in interest, in thought and in conduct. "The typical kindergarten and the typical Young Men's Christian Association illustrate what is thus far the best statement of the problem of religious education and constitute the most consistent attempts at its solution."

F. SMITH.

Die Kultur der Gegenwart, herausgegeben von PAUL HINNEBERG. Teil I, Abteilung V. *Allgemeine Geschichte der Philosophie.* Berlin und Leipzig, B. G. Teubner. 1909. pp. viii, 572. Price Mk. 12.

The present volume of this comprehensive work is made up of eight essays, excellently proportioned, which cover the main divisions of a systematic history of philosophy. A general introduction, on the beginnings of philosophy and the philosophy of primitive peoples, is contributed by Wundt. The four following chapters are devoted to the philosophy of the Orient, Oldenberg writing on India, Goldziher on Islam and Judæa, the late Professor Grube on China, and Inouye on Japan. Occidental philosophy is treated under three principal headings: von Arnim is responsible for the account of ancient philosophy,—and Bäumker and Windelband deal, respectively, with the medieval and the modern periods. Every section is therefore written, not only competently, but with authority, and the editor is to be congratulated upon his choice of collaborators and his success in securing their co-operation. The value of the work is greatly enhanced by the selected bibliographies appended to the successive chapters; and there is a good index.

It goes without saying that the volume contains much that is of interest to psychologists; it furnishes, on many counts, materials of high value towards that history of psychology which is still to be written. We must here confine ourselves, however, to a brief account of Wundt's paragraphs upon primitive psychology.

Wundt begins by emphasizing the unity, to primitive observation, of the bodily and the mental life. The notion of psyche, a mind or soul, could hardly have arisen, were it not for two phenomena which bear witness to a separation of mind and body, namely, death and dreaming. Once the notion has taken shape, we find three main trends of primitive thought. If the idea of the unity of life is dominant, we have the bodily soul (*Körperseele*), a soul which remains in the body after death, or returns to it after intervals of absence. This conception gives rise to two secondary conceptions: to the idea that the first worm which leaves the dead body carries the soul with it (*Seelenwurm*), an idea that may account for the long-continued belief that the departed soul finds embodiment in a snake; and to the idea of a seat of the soul in various bodily organs, the doctrine of *Organseelen*. If, again, the idea of death is dominant, we have the breath-soul (*Hauchseele*). The departing spirit, *psyche*, *anima*, *spiritus*, *Geist*, *ghost*, leaving the body on the last breath, then finds embodiment in animals, particularly in creeping, fluttering, or quick-moving forms. We are here at the beginning of the belief in metempsychosis, as we are also on the path which leads to totemism. If, finally, the idea of dream is dominant, we have the shadow-soul (*Schattenseele*), the *eidolon*, *umbra* or shade. This shadow-soul acquires a certain stability, in the primitive mind, partly from the frequent recurrence of dream-experience, and partly from what we may call the ratification of that experience in states of disease, more especially in the delirium of fever.

In reality, these three ideas occur only in combination, although now one and now another may receive the greatest emphasis. In the last resort, the shadow-soul gains the upper-hand. The notion of a circumscribed seat of the soul, in some particular bodily organ, serves, so to say, to break up the general bodily soul, to free it of its dependence in totality upon the body. The soul accordingly passes from the body, at death, with the breath. This breath-soul, in its turn, changes to the insubstantial shadow-soul familiar to us, *e. g.*, from the Homeric psychology. Finally, the shadow-soul attains to complete independence; it ceases to be considered as the soul of some dead person, and assumes an existence in its own right. As ghost or demon it becomes a natural force, terrible or protective. In this way primitive psychology passes over without break into a primitive philosophy of nature.

This summary account is, of course, very far from doing justice to Wundt's exposition. It presents, however, an outline of the views which, in the original, are set forth with all the convincing force of a practised literary style. Even there, much of detail had necessarily to be omitted; Wundt's theory of totemism must, for instance, be sought elsewhere. On the other hand, space has been found for a number of interesting facts. Thus, as regards the seat of the soul, we are told that the oldest *Organseelen* resided in the kidneys and the blood. The importance of the blood is obvious; that of the kidneys Wundt finds in their close association with the organs of sex. The placing of the soul in the saliva is explained by reference to the breath-soul. Later, the soul has its local habitation in diaphragm and heart; here, evidently, the expression of emotion is the key to the situation. One wonders, indeed, whether the phenomena of emotive expression were not the starting-point of all concrete psychological observation; whether, if death and dream were the occasion of psychology, emotion and its expression were not the occasion of the first efforts to psychologise.

TH. WALTERS.

BOOK NOTES.

Das Sinnesleben der Insekten, von AUGUST FORREL. Ernst Reinhardt, München, 1910. 391S.

In this work this veteran author and investigator in the field of the psychology of instinct brings together the general results of his own investigations, which chiefly touch the sense of sight, smell, taste, hearing, touch and its derivative senses, the relations between the senses and the mental powers. He also reports further experiments on the sense of sight of ants, criticises experiments on sight, color, and distance sense of insects published since 1887; then treats of orientation in space, the power to communicate, the memory of time and place, with a final chapter on the soul and reflexes.

Vorlesungen über Tierpsychologie, von KARL CAMILLO SCHNEIDER. Wilhelm Engelmann, Leipzig, 1909. 310S.

The author is extraordinary-professor of zoölogy at Vienna. The contents of this rather stately book will be sufficiently indicated by giving some of its twenty-one chapter heads. They are printed in the form of lectures: animal psychology with and without a soul; the powers of distinction and formulation of problems, protozoa and cnidaria, star-fish, worms, mollusks, crustacea, insects (four lectures), vertebrates (eight lectures). In a final chapter the author seeks to describe the structure of the *geistig* world, in which he brings together his own special theories.

Ant Communities and How They Are Governed. A Study in Natural Civics. By HENRY CHRISTOPHER MCCOOK. New York, Harper & Brothers, 1909. 321 p.

This is an interesting, rather systematic and yet popular work with nearly a hundred cuts and illustrations and at the end a copious bibliography upon the subject. It treats fraternal confederacies, nesting, architecture and engineering, supplying of rations, feeding the commune, the language of ants and other insects, female government, problem of communal dependence, of warrior ants and their equipments and how they carry on war, alien associates, aphid herds and associates, the founding of slave making, problem of sanification and personal benevolence. From this very title it will appear how dominant is the author's purpose to present his theme as a study of natural civics.

Tierwanderungen und ihre Ursachen, von FRIEDRICH KNAUER. J. P. Bachem, Köln, 1909. 288 p.

After describing the general causes of migrations, salient instances are described among mammals, birds, and the lower animal forms. All together it is a compilation of material that has long been desired.

The Mendel Journal, No. 1, October, 1909. Taylor, Garnett, Evans & Co., London and Manchester. 216 p.

It is assumed that Mendelism is a subject which has come to stay and to play an important part in human affairs; whether in agriculture, horticulture, cattle raising, or sociology, its voice will be heard.

It is vastly complex, but it has already some practical importance. This journal is to gather for the science of genetics a harvest rich in facts relating to human pedigrees and the inheritance of normal characters as well as peculiarities. It is to give prominence to agricultural and horticultural problems also. The first number contains very interesting articles on human pedigrees, the evolution of man; there is a plea for a more virile sentiment in the administration of human affairs; an interesting chapter on the relation between Mendelism and sex, together with reviews and book notes. Altogether this journal makes a very interesting and attractive first appearance.

L'Evolution de la Mémoire. Par HENRI PIÉRON. Paris, Ernest Flammarion, 1910. 360 p.

This author first treats of the inorganic persistencies of human memory in general, then considers rhythmic persistencies, first in the vegetable and then in the animal world. In the second part, animal memory is treated and he describes the various experimental methods showing adaptation and the acquisition of habits, censorial memory. In the third book he comes to man, discusses first of all the modalities of memory, the stages of acquisition, conservation, recognition, evocation and localization. The varieties of memory he classifies as ethnic, individual, ontogenic and pathogenic, and then turns to its utilization in education. The final chapter is mainly devoted to what the author calls the socialization of memory.

Attention and Interest. A Study in Psychology and Education. By FELIX ARNOLD. New York, The Macmillan Company, 1910. 272 p.

This book considers first attention in relation to a given situation, its objective psychophysical physiological aspects. The second deals with interest, its motor and ideal aspects. And last comes education with attention and interest in the schoolroom. The author attempts to clarify and arrange the many facts that have been brought to light by numerous experiments in the psychological laboratories. He does not hold to any special theory or school but seeks to present the facts as they are.

Der Wille. Versuch einer Psychologischen Analyse. Von ELSE WENTSCHER. Leipzig, B. G. Teubner, 1910. 189 p.

The author begins with reflex action and that of animals and then discusses successively the motive of will, the analysis of the act of will, its development in child life, its relation to thought, its function in moral conflicts, its energy, its freedom, and the modes of analysis.

Vorlesungen über die Psychopathologie des Kindesalters für Mediziner und Pädagogen, von WILHELM STROHMAYER. H. Laupp'schen Buchhandlung, Tübingen, 1910. 303 p.

The author, who is a *privat docent* in Jena, after a few general lectures treats of neurasthenia and chorea, hysteria, epilepsy, weak-mindedness and its symptoms and treatment, and moral insanity. It is a comprehensive and excellent work, with a good bibliography appended.

A Text-Book of Mental Diseases, by EUGENIO TANZI. Authorized translation from the Italian by W. Ford Robertson and J. C. Mackenzie. London, Rebman Limited, 1909. 798 p.

This is an admirably lucid and comprehensive treatise which is quite up-to-date in most respects and constitutes a very valuable addition to the literature available for experts who read English. The chief chapters are as follows:—the seat of the psychical processes, the causes of mental disease, their anatomical substratum, sensibility,

ideation, memory, the sentiments, movements and other external reactions, classification, pellagra, alcoholism, amentia, thyroid psychoses, progressive paralysis, involuntary cerebropathies, including acquired idiocy, the cerebropathies of adults, affective psychoses, neurasthenia, hysteria, epilepsy, dementia præcox, sexual perversions, constitutional immorality, paranoia, imbecility and asylums.

The American People. A Study in National Psychology. By A. MAURICE LOW. Houghton, Mifflin Company, Boston and New York, 1909. 446 p.

The author calls the American people a new race in a new environment, undergoing climatic amalgamation. He shows how Puritanism is at the root of our nationality and how it gave birth to democracy and became a political force. Thus the American has always been a rebel who, although basing his creed upon the Bible, hated all myth and mythology. Outside this circle, Virginia represented an aristocratic oligarchy. Here the first Catholic colony was founded. Rice produced new social conditions. Religious freedom is born here.

Music, its Laws and Evolution, by JULES COMBARIEU. Authorized translation. Kegan Paul, Trench, Trübner & Co., London, 1910. 334 p. (The International Scientific Series.)

This work is attractive to psychologists. The author first treats musical thought and psychology, including the analysis of a melody, examination of opinions, musical images and their organization, music and magic. The second part deals with music and social life, describing apparent antinomy, rhythmic elements, relations to love, speech and society. The third part deals with musical thought and physiology, the function of the ear, localization of images, musical intelligence; and the last part with musical thought in its relation to nature, including its mathematical aspects and its effects upon living beings.

Précis d'Auto-Suggestion Volontaire. Par GÉRAUD BONNET, D'ORAN. Paris, Jules Roussset, 1910. 295 p.

The author starts in by discussing hypnotism and auto-suggestion, then considers education of the will, its relation to self-confidence, concentration of thought and personal power. The work is practical in its nature and seeks to develop good qualities both of moral action and of industry and perseverance.

The Classical Moralists. Selections illustrating ethics from Socrates to Martineau. Compiled by Benjamin Rand. Boston, Houghton Mifflin Company, 1909. 797 p.

This is a companion volume to the author's Modern Classical Philosophers and is a history of ethics based, not on ordinary descriptions of systems, but on selections of, and translations from, original sources. It begins with Xenophon's account of Socrates and ends with James Martineau and considers altogether forty-five ethical authors.

Knowledge, Life and Reality. An Essay in Systematic Philosophy. By GEORGE TRUMBULL LADD. New York, Dodd, Mead & Company, 1909. 549 p.

This voluminous author comes to us with another large and comprehensive work beginning at the very foundations of things. The conceptions and problems of philosophy, its methods, schools, philosophy of knowledge, etc. He then takes up kinds, degrees and limitations of knowledge, its principles and presuppositions, scepticism, agnosticism and criticism, metaphysics as a theory of reality,

nature and significance of categories, philosophy of nature, mind and its relations to matter and spirit, ethics, its sphere and problem, the moral self, the morally good, schools of ethics, ethical consciousness, the arts, their classification and nature, the spirit of reality, philosophy of religion, its origin and experiences, the world-ground as an absolute person, God as ethical spirit, his relations to the world. The purpose of the book, we are told, is best explained by the life work and life purpose of its author, who for more than a generation has observed, read, taught, in this field. During all these years he has come to Plato's viewpoint that philosophy and love are akin.

The Philosophy of Happiness; a Consideration of Normalism. By R. WAITE JOSLYN. Elgin, Illinois, Normalist Publishing Company, n. d. 200 p.

Normalism is a philosophy of life that is independent of creed or doctrine, but assumes that there is a normal way of conduct the realization of which brings man to his height. The book has no index, the chapters are unnumbered, and we have been utterly unable to read it.

The History of Medieval Philosophy, by MAURICE DE WULF. Third edition. Translated by P. Coffey. London, Longmans, Green & Co., 1909. 519 p.

This is made from the third edition of the author. There is first an historical introduction on Grecian and Patristic philosophies, which occupies the first hundred pages of the work. Then after introductory considerations, the first chapter ends with the twelfth century. The thirteenth century makes a period of itself. The third comprises the fourteenth and the first half of the fifteenth centuries, and the fourth ends with the seventeenth century. It is impossible in the space at our disposal to do justice to this scholarly book. Suffice it to say that it bears throughout the mark of great erudition and is extremely lucid. It is, of course, written from the Catholic standpoint but is a valuable addition to our knowledge in this field.

Distribution and Movements of Desert Plants, by VOLNEY M. SPALDING. Published by the Carnegie Institution, Washington, D. C., 1909. 144 p.

The writer first describes habitats; then considers local distribution, environmental and historic factors, various groups, desert laboratory domains, origin of desert flora, etc. The book is copiously illustrated by photographs, charts and maps, and cannot fail to be of great interest.

Jahrbücher für Psychiatrie und Neurologie, herausgegeben vom Vereine für Psychiatrie und Neurologie in Wien, redigiert von Drs. Fritsch, Krafft-Ebing, Obersteiner, Pick, und Wagner v. Jauregg, unter Verantwortung von J. Fritsch. Vierzehnter Band. Franz Deuticke, Leipzig und Wien, 1896. pp. 275-578.

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DIVERSE IDEALS AND DIVERGENT CONCLUSIONS IN THE STUDY OF BEHAVIOR IN LOWER ORGANISMS¹

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The living interest of the study of the behavior of animals lies in the concrete facts: in what the animals *do*. A kine-matograph would perhaps make the best possible presentation of this subject. But to-day I am going to deny myself the pleasure of presenting the concrete facts, and deal rather, since this is a conference on research, with certain problems of investigation, in the behavior of the lowest organisms. I wish to try to present the main general results of investigation in this study of behavior at its lowest terms, together with its relation to certain general theories; to show why there is such marked disagreement in the accounts given by different investigators; and to point out the fundamental problems for future work. I realize only too strongly that I am not a psychologist, and that my claim to a hearing before psychologists lies only in that my work has been with matters which the psychologist needs to take into consideration. I shall therefore not deal with matters that are psychological in the subjective sense, but with some of the biological relations that underlie psychology.

In recent years a new spirit, a new desire, has permeated biological science in every division,—in brief, the desire to *see the processes of nature occurring*, and to modify and control these processes—not merely to judge what processes *must*

¹ Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

have occurred.¹ In the words of the young Clerk Maxwell² we wish to see the "particular go" of the processes of nature. This is the essential point in the present wide use of the experimental method in biology.

Contrasted with this is an earlier method of work, which may be expressed as follows: Certain conditions were seen to exist. From this, conclusions were drawn as to what *must have occurred*, in order that these conditions might exist. If we succeeded in imagining a process that would satisfactorily account for what exists,—then that was a sufficient explanation. If we found further that this explanation fitted certain other facts, which it was not devised to explain, then it was felt that the explanation was confirmed; was *verified*, even though the supposed process had not itself been observed.

This method of interpretation was long the common one throughout the general biological sciences, zoölogy and botany, and was much in use even in physiology and psychology. The great example is the theory of evolution, together with the special theories grouped about it, concerning the origin of particular organisms, or of particular structures and functions.

In the new spirit of work, the desire is to see things happening, not to conclude what must have happened. If evolution occurs, we wish to *see* it occurring; if acquired characters are inherited, we want to see a few acquired and inherited. We wish to see the processes themselves, not merely the result of supposed processes.

This is the spirit that has led to the recent rebellions against accepted doctrines. Its more thorough-going partisans reject all explanations that are merely devised to explain results. If they can't see evolution occurring, they conclude that it does not occur. They refuse to accept the principle of selection because they do not see it at work. They tell us, with von Uexküll,³ that Darwinism is to be stricken from the list of scientific theories; with Driesch,⁴ that Darwinism fails all along the line. Any explanation that deals with processes not observed is ruled out. The ideal is to build up an account of nature that shall consist entirely of statements that can be verified; that is, in which any process said to occur can be *observed* to occur, if the conditions are properly supplied.

¹ Compare the statement of the object of scientific investigation by Loeb, to whom perhaps more than to any other person the prevalence of this ideal is due. (Loeb, J., Preface to *Studies in General Physiology*, 1905.)

² As quoted by James, *Pragmatism*, p. 197.

³ Die neuen Fragen in der experimentellen Biologie. *Rivista di Scienza* "Scientia," 1908, vol. 4.

⁴ Science and Philosophy of the Organism, vol. 1, p. 269.

It is only by grasping thoroughly this ideal that one can understand the basis for the views set forth by its exponents; the startling conclusions drawn by such men as Driesch, v. Uexküll and Loeb.

The same spirit and method have been carried into comparative psychology, and the first result was to banish that which had been supposed to constitute the very subject under examination. "Comparative psychology" became "animal behavior," for psychological processes cannot be observed in animals, and are therefore ruled out. Their very existence was called in question, and all discussion of them was denounced as idle speculation; comparative psychology was denied a place in science.¹

This spirit has doubtless led to iconoclastic excesses in its zealous partisans; there has been a tendency to forget that within the next ten thousand years many processes may be observed that we have not succeeded in finding in the decade or two in which this spirit has ruled, so that sweeping negative conclusions are hardly warranted. But as a basis for a positive working method the ideal is sound and valuable. We must observe the processes of nature, not merely guess at them. Certain results and problems arising from the study, in this spirit, of the behavior of lower organisms, is what I wish to bring before you. Let us accept the tests imposed by this ideal, and see what we have reached.

Strangely, perhaps, this spirit of work has not succeeded in bringing about agreement in the study of behavior in the lower organisms. As you know, extreme divergences on what seem fundamental matters are found between the accounts given by different sets of experimental investigators in this field. Certain investigators show the phenomena as simple and dominated by easily understood mechanical factors; concrete physico-chemical explanations are presented as throughout satisfactory. The whole matter is set forth in such a way that he who runs may read and understand. Here, one feels, is a field that has been set in order; here the physico-chemical methods of attack have shown themselves adequate. And what a contrast to the results of older methods of study! No psychological discussions, no anthropomorphism, no teleology; no uncertainty, but simplicity, uniformity, constancy everywhere; simple mechanical considerations suffice for all. Truly a triumph of the mechanistic views of life phenomena!

But to the student's perplexity, he finds another group of

¹See for example, v. Uexküll, *Im Kampf um die Tierseele*, reprinted from Asher and Spiro's *Ergebnisse der Physiologie*, Jhrg. 1, 1902, and Nuel, *La Psychologie Comparée est-elle Légitime?* *Archives de Psychologie*, T. 5, 1906, pp. 326-343.

experimentalists, working with equal thoroughness, that give a very different account of these matters. They tell us that behavior in the lower organisms is extremely complex, varied and variable. The simple mechanical explanations are largely rejected as unverifiable and inadequate to the facts. The regulatory character of behavior, which has given rise to teleological and vitalistic doctrines, is insisted upon. Resemblances are pointed out between the behavior of lower organisms, and that of higher animals and man. The striking differences between the behavior of even the lowest organisms, and that of inorganic bodies is set forth.

The members of the first group of investigators thereupon accuse those of the second group of most grievous sins. They accuse them of indeterminism, of anthropomorphism, of "psychologizing," of teleology or finalism; of vitalism.¹ The members of the second group plead not guilty to these charges, and are inclined to respond by characterizing the work of the first group as superficial, misleading and generally inadequate.

Why this divergence? Why should investigators working on the same field under the same principles fail in this radical way to come to agreement?

The divergence appears to me largely due to certain differences in the plan and method of investigation; to difference in opinion as to what knowledge concerning behavior is of worth. There are two diverse ways of attacking the problems of behavior in lower organisms, and these two ways lead naturally to the two divergent sets of views that we have sketched.

1. The first method, and the one giving the brilliantly simple results, may be called the method of the physico-chemical key, or from certain points of view the *synthetic* method. It consists most typically in taking some single physico-chemical principle, or the action of some simple physical agent, and using this as a key to unlock the secrets of behavior. One takes osmotic pressure, or surface tension, or the electric properties of ions, and traces its operation throughout behavior; or he studies the direct action of gravitation, of heat, of light, on the movements of animals. That is, one keeps his eyes on this physico-chemical principle or agent; this, not the organism, forms the unit of work, the object of investigation. Experimentation consists in subjecting organisms to the operation of the principle or agent in question. When we have thus traced the action of the various physico-chemical principles or agents with which we are acquainted, we have the science of behavior; this science is thus built up synthetically from the simplest elements.

We have all seen this method applied, with brilliant results,

¹For a compendium of such accusations, the recent book of Dr. Bohn may be consulted (*La Naissance de l'Intelligence*, 1909). See also Loeb, *Journ. Exper. Zool.*, vol. 4, 1907, pp. 151-156.

in many fields of biology. It has given us in behavior, among other things, the famous tropisms,—the direct, uniform effect of constantly acting physical agents on living organisms.

Those who work in this way commonly hold that it is the only method of work that is worth while. The purpose of investigation is held to be, to work out the physics and chemistry of biological phenomena. The way to do this is evidently to take our known physical and chemical laws, and trace their operation in the biological field. A fundamental principle for this method of work is this: physico-chemical action is constant; it can be depended on. With the same reagent acting on the same material, we must get always the same results. The organism is the material; the reagents to be used are the known chemical and physical ones; the results form our science of behavior.

This is the method of investigation which gives the plain and simple results. The ogres of anthropomorphism, of teleology, of vitalism, that have devoured so many biologists, nowhere so much as show their heads to the traveller upon this route.

2. The second method of study may perhaps be called the analytical method. It is based upon interest in the organism rather than on interest in physics and chemistry, and it makes the organism the unit of work, the object of investigation. The investigator wishes to know all about a given organism. Among other things, he wishes to know its entire behavior, and incidentally whether its behavior is as a matter of fact fully accounted for by known physico-chemical principles, and if not, how much and what is left over.

The actual method of work is to first watch the organism under its natural environment, till one finds out all things it does. Then the environment is changed a little, to see what difference this makes in the behavior. We thus try all sorts of different ways of getting the animal to change its behavior,—including the application of definite chemical and physical reagents of most varied character. We find all the different things the animal can do, and we work out the determining causes and conditions for each. We find all the different ways in which the animal may react to the same stimulus, and we find all the different methods of causing it to give each reaction. We thus find the organism's system of behavior and the things that influence it,—becoming acquainted with the creature as we might get acquainted with a person with whom we are thrown much in contact.¹ We carry on the same sort of work with different organisms, and compare them.

¹ Compare v. Uexküll's statement that the first requirement is "Die andauernde und eingehende Beobachtung des lebenden Tieres in seinem Milieu" (*Leitfaden in das Studium der Experimentellen Biologie der Wassertiere*, Wiesbaden, 1905, p. 75.)

Thus in this method we begin with the complex organism and attempt to analyze its behavior, proceeding to simpler and simpler determining factors, till we get the simplest that can be reached. In this way we must of course finally reach the simple physical and chemical factors with which the investigators by the other method start, provided that the latter are real factors. But since the analysis is difficult and slow, for a long time we are forced to deal with complex components; we separate the original complex mass into smaller and smaller complexes, each to be thoroughly analyzed later. These complex components are not elementary physical or chemical factors. We deal with organisms as wholes, and with such concepts as respiration; the securing of food; protection and defense; care of the young, construction of nests and the like; things which cannot now be expressed in the terminology of physics and chemistry.

Thus the investigator by this analytical method does not agree that work with the elementary chemical and physical factors is the only thing worth while; he finds, in contrast to the worker by the synthetic method, that the relations between these complex, biological components are in themselves decidedly worth while; that they are indeed of the greatest interest and importance. Such are the relations of organisms to each other; the preying of one on another; the conjugation of organisms; the selection of food, and other relations of behavior to metabolism; the relation of behavior to defence and protection, the similarities and differences between the behavior of different organisms, and the like. These things are not physico-chemical concepts, and the relations between them fall outside the field of view of the investigator who, in the synthetic method, works only with such concepts.

These biological interrelations form then a large part of the field of study by the analytical method. But it is of course clear that there is nothing incompatible in all this with further analysis into elementary physical and chemical factors. We merely find it impracticable and useless to wait until such analysis is complete before dealing with the important biological interrelations. Indeed, we should have to deal equally with these biological relations even if the analysis into physico-chemical factors were completed; they would be quite lost sight of if we limited ourselves to an account of the simple chemical and physical factors. The interrelations of mountain, stream and forest are important, but they are not easily set forth in terms of the movements of electrons or ions; and the working of a Hoe printing press is an equally refractory subject for presentation in that way. As Ritter has well emphasized in a recent paper "you can never give a full account of any whole in terms of its elements."¹

¹Ritter, W. E.: *Life from the Biologist's Standpoint*. Pop. Sci. Monthly, 75, p. 177.

But of course the work of analysis is not finished, in the case of the organism, as in the landscape or the machine, until each part is resolved into the elementary determining factors with which the synthetic method starts.

Thus the synthetic method starts at the bottom and attempts to see how the complex behavior is built up from the simplest elements, while the other method starts with the given complex,—the organism and its behavior,—and attempts to resolve this into its elements. It is of course never possible to classify men absolutely, but typical examples of the different sorts of investigators can be given. For the synthetic method, beginning with the simple chemical and physical principles, the great example is of course furnished by Loeb and his followers. For an example of the investigators on lower animals that begin with the complex and attempt to analyze it, we may name von Uexküll. We may cite also the brilliant work done on behavior in lower organisms under Professor Hodge's direction, in this university. The present speaker has likewise followed this analytical method; and it has almost of necessity been the method employed by workers on higher animals.

Now, unless we are mistaken in our fundamental premise that physico-chemical explanations can be given for behavior, both these methods of work are quite justified, and they must finally, when they have finished their work and covered the ground thoroughly, come to the same results. But one method begins at one end, the other at the other end, and since "science moves but slowly, slowly," we find that at any given time the two have not met; they have not covered the same ground; they do not see the field alike,—and there arise misunderstandings and controversies.

The first method of exploring behavior may be compared to the method of the prospector who goes into a new country seeking gold. His eyes are all for the signs of gold; he may find it and come back laden, as Loeb has done again and again. But he is not so likely to bring with him a correct map of the country as is the surveyor.

The second method of exploring behavior may be likened to that of the geological survey. The first business is to make a topographical map of the country, so that we may guide ourselves and get a general view. The next is to make a systematic and detailed examination of all regions, to find out the relation of river and mountain and forest; of formations, strata and soils.

The surveying party is not so likely as the prospector to return from a given trip with gold, but it is naturally more likely to have made an accurate survey of the country, and to

have determined the various things that are there to be found. It may even be able to correct certain distorted views reported by the prospector. The investigator who works in this way is likely to give a more adequate 'natural history' of behavior than the worker by the other method. He will have a more complete idea of the matters remaining to be worked out, the difficulties to be met, and the relation of the different parts to each other.

It is the difference in the pictures presented by the men working in these two diverse ways that is the main cause of the controversies that have arisen. In other fields of biological investigation we find the same two methods of work, and there appears to be an almost "irrepressible conflict" between their representatives, though the divergences in results are perhaps less crying than in behavior. My present purpose is to try to compare in certain general features the picture presented by the two different sets of workers, in the behavior of lower organisms, pointing out the ground for the divergences.

In the unicellular organisms we have theoretically the simplest condition of affairs that we can find; we have the problem of behavior at its lowest terms. What are the characteristics of this behavior? What bearing have the phenomena here on the possibility of giving physico-chemical explanations for natural phenomena? What relation have they to those three reprobated tendencies or doctrines of which we hear so much of late, in attack and defence, in accusation and recrimination: to vitalism, to anthropomorphism, to teleology or finalism? Why do these doctrines maintain themselves, and how far is there justification for them?

Now, it is evident that the first or synthetic method of study, from its very nature, avoids any phenomena on which these reprobated doctrines could possibly be based, any phenomena not manifestly of a physico-chemical character. It simply does not look at them. It devotes itself to visible physical and chemical matters, leaving the rest out of the field of view. This is done deliberately and intentionally, because it does not consider other matters ripe for treatment. The point of view of such investigators is well expressed by Bohn, in his recent eulogy of Loeb: "as a man of positive science, he does not waste his time reasoning about things that have resisted scientific analysis."¹ I believe that no criticism can be made of this procedure, as a method of work, provided it does not represent as non-existent the parts with which it does not deal. Its chief danger is that it tends to make one forget how much remains

¹"—En homme positif, il ne perd pas son temps à raisonner sur des choses qui ont résisté à l'analyse scientifique." Bohn, *La Naissance de l'Intelligence*, 1909, p. 44.

to be done. To point out the limits of our present knowledge and the problems that remain to be solved is almost as important as to set forth what has already been worked out. And this is particularly true in fields where that which is understood is but a fraction of that which exists. The investigator indeed must keep his eye on what remains to be done, rather than on what has been done.

Thus the freedom from anything vague or uncertain; complex or difficult to understand; anything not resembling common physico-chemical action; anything that could suggest anthropomorphism or finalism or vitalism,—that we find so striking in the accounts of investigators working by the synthetic method, is after all *a priori*, and due simply to the omission of everything of that sort. The reader, finding in these accounts nothing to suggest the difficulties, concludes that the difficulties do not exist; that this is a field where the resolution into physics and chemistry has become complete.

But the worker by the analytic method is unable to escape so easily from the difficulties. Making the complex organism his object of study, interested in this for its own sake, and attempting to omit nothing, he finds himself at once confronted with a mass of phenomena that have not been analyzed into physico-chemical factors; phenomena in the highest degree complex, varied and peculiar. He is even pained to find that many of the proposed physico-chemical explanations are inadequate for the very phenomena which they have been called in to explain. He is forced to deal with that storehouse of materials from which anthropomorphism, finalism and vitalism have drawn their supplies. He endeavors in a preliminary way to analyze and arrange these, and to report to those interested, what one finds when one examines thoroughly the behavior of lower organisms. If he is honest, he is compelled to report even in the lowest organisms many features that resemble features in the behavior of man. He finds inescapable relations between the present actions of the organism and certain later conditions,—resembling what is seen in the purposive action of man. Though these discoveries may be unwelcome, the surveyor with a conscience is forced to set them down on his map and mention them in his description.

And then the storm bursts upon his head. Anthropomorphism! Teleology! Finalism! Vitalism! cries in horror the aggressive physico-chemist, and the rash investigator is drummed out of the mechanistic camp. The teleologist and the theologian seize upon the account of actions resembling those we perform with a purpose, and count the author among their allies. The upholders of vitalism hold out the right hand of fellowship to

the investigator who has shown the inadequacy of physico-chemical explanations.¹

Yet the unfortunate investigator whose sin has been to try to tell the whole truth is still a believer in the validity of physico-chemical explanations; in the necessity of formulating all processess causally; he holds that teleology cannot be substituted for causation, that to perceive the resemblance between man and lower animals does not give a causal explanation of either; he is convinced of the error of vitalism. Rejected from the camp of the physico-chemists, and refusing to take refuge with the enemy, he is left an outcast.

Where is the mistake? Do such accounts of behavior as he gives really imply that physico-chemical explanations are wrong in principle,—as both the vitalists and the aggressive physico-chemist seem to agree that they do?

If they do—alas for physico-chemical explanations—for such accounts are certainly correct! But how incredibly short-sighted; how faint-hearted, is the physico-chemist that takes such a view. These accounts of behavior neither justify finalism and vitalism as substitutes for causal explanations, nor do they refute mechanism. They show merely that there is still much work for us to do; that the end of analysis is by no means in sight; that some of the methods of analysis have been inadequate; that the problems are much more extensive and difficult than some have imagined. They have no more weight in overthrowing physico-chemical explanations than has the existence of an unexplored country, in showing the impossibility of exploration. True, the choice between physico-chemical and vitalistic formulations must for the present, for a large part of the phenomena at least, be based upon a *a priori* grounds, not upon demonstration. But whoso has imagined otherwise should remember the saying of Newton, that he had but picked up a few pebbles on the shore of the ocean of knowledge, and should ask himself whether he has really gathered into his pocket that measureless ocean and all its shores. When he has done that, he may have established by demonstration the complete adequacy of physico-chemical explanations; until he has done it, we must rely upon a *a priori* considerations.

If we accept from beforehand the programme of physico-chemical or mechanistic explanation, how are we to conceive the facts in the behavior of lower organisms? What are the chief facts discovered in a careful survey; what is their relation to vitalism, to anthropomorphism, to teleology?

¹ See for example Driesch, *The Science and Philosophy of the Organism*, vol. 2, and v. Uexküll, *Die neuen Fragen in der experimentellen Biologie*, *Rivista di Scienza* "Scientia," vol. 4, 1908.

1. Let us consider vitalism first. The experimental investigator is interested in vitalism on account of its practical bearing on the justifiability of his methods of work. He can experiment only by altering in some way the configuration of matter and energy: *i. e.*, by making some physical or chemical change. Can he hope to carry through this method consistently and completely, explaining all divergences in results by differences in the preceding configuration of matter and energy? Or will he find cases of divergence in results where there are no foregoing differences in the configuration of matter and energy, so that from throughout identical configurations diverse results follow? If the latter is the case, then evidently the experimental method fails.

The question here is not merely whether the explanations hitherto commonly used in physics and chemistry are entirely adequate ones. It is the deeper question, whether when adequate principles of explanation *are* used in the physics and chemistry of things not alive, other and diverse principles of explanation will be required for the study of living things. The question is as to whether there is a real, fundamental diversity between the necessary principles of explanation for living and for non-living things.

To hold that the necessary principles of explanation are not diverse in the two cases; to hold then, in opposition to vitalism, that physico-chemical principles of explanation are generally adequate, is to hold this:

What happens in any system of matter and energy at any period is determined by the configuration of matter and energy at a preceding period. Experimentally, therefore, differences in resulting conditions in any two cases will always be found preceded by differences in foregoing conditions, so that nothing happens without its determining factors in the previous configuration of matter and energy. If we search with sufficient care, we shall always be able to find in matter and energy a determining factor for everything that occurs. Two identical combinations of matter and energy cannot produce different results, nor two different combinations absolutely identical results.

This of course leaves open the question whether there may not be present in certain cases additional, subjective, properties, and also, whether it may not be proper to make, besides the physico-chemical explanations, other explanations that take into consideration these subjective properties. Furthermore, it does not deny that there are in living things combinations of matter and energy not found elsewhere, so that methods of action may occur that are not found elsewhere; to this we return later.

In attempting to understand and explain the behavior of

the lower organisms, one of the chief stumbling blocks has been a preconceived idea as to what we should find. Many have imagined these creatures to be a sort of link between organic and inorganic material. Higher animals it is generally recognized are complex structures, comparable in this respect to machines. In the lower organisms it was expected that we should find mere masses of a certain sort of material, and the study of behavior would be merely the study of the chemistry and physics of this material. This idea has perhaps been most explicitly developed by Le Dantec.¹ This *a priori* conviction, often unformulated, maintains itself most obstinately in all thought on the behavior of lower organisms, particularly of those composed of but a single cell.

The most fundamental result of the study that has been made is to show the incorrectness of this idea; to show that lower organisms, like higher ones, are typical *arrangements* of material; are structures; are in this respect machine-like; not masses of a uniform substance.

This is evident both from a study of the activities of these organisms, and from a direct examination of their structure. Such a unicellular creature as Stentor has many systems of differentiated organs, often acting independently of one another; its structure can only be characterized as inconceivably complex. Paramecium with the microscope is seen to be a most complicated machine, running at a high rate of speed. In studying the behavior of these creatures, what we find out is *how certain machines work*, rather than the direct physical and chemical properties of a certain substance.²

The practical difference in the results of study, due to this fact, is perhaps much greater than appears at first thought. To this difference between what was expected and what was found are due most of the paradoxes and difficulties in behavior, and most of the apparent failures of direct physico-chemical explanations. To it are due the recrudescence of anthropomorphism, finalism, vitalism. Let us try to see how these things come about. In so doing I shall have to remind you of certain facts that are sufficiently obvious, yet that have been neglected of late in work on lower organisms.

The first principle of physico-chemical explanations is that the chemical and physical properties of the living substance

¹ *La Matière Vivante*. Paris, 1895.

² Whether, from other points of view, an organism is not also something more than a machine, is another question. If we mean by a machine any system in which the configuration of matter and energy determines what happens, then the contention in the text is that for purposes of causal explanation of what happens only this machine-like character requires consideration.

determine its reactions, so that from a knowledge of these properties we can predict the reactions; and further that the same reagent acting on the same substance is bound to produce the same result. Any statements to the contrary are looked upon as controverting the principle of causal determination and leading to vitalism.

But when the substance is arranged in typical ways, this principle, though essentially true, becomes practically false and misleading. From the same mass of substance we can make many different arrangements or machines, acting in entirely different ways, so that we could never predict the reactions of the machines from a knowledge of the chemical and physical properties of the unarranged substance. From a certain mass of material we could make either a clock or a doorbell or a steel trap or a musical instrument,—and we could easily so arrange these that each would respond in its characteristic way when acted upon by an electric current. We could, moreover, make the same machines, showing the same reactions, from a different kind of material, with different properties. We could then never predict the reactions of these by knowing merely the chemical and physical properties of the material of which they are composed. The specific action of each depends on the specific arrangement of its material.

This is exactly what we find in organisms, including the lowest as well as the highest. From it there result certain relations that are extremely perplexing, though they can be illustrated from inorganic combinations as well as from organic ones. Let us examine some of these.

First, both in inorganic arrangements or machines and in organisms we find that the same substance reacts to a given reagent sometimes in one way, sometimes in another. It all depends on how the material is arranged;—whether as a clock or as a doorbell; whether as a Stentor or as a Paramecium. Furthermore, by a slight shifting of the arrangement, we find that the very same piece of material is caused to react to the same reagent in entirely different ways. A typewriter responds to mechanical stimulations by printing English lower case letters. After momentary pressure of a certain lever, it responds to the same stimulations by printing capital letters, or by printing numbers or conventional signs. It would be easy to so arrange it that after a shift of a lever, it would respond with script or italics or German or Russian or Greek letters. Here we have parallel conditions to what we find in lower organisms. What we find to be true for one organism or one individual turns out not to be true for another, and even what we find to be true for a given individual will not hold later for the same

individual. The principle of the *shift* in arrangement comes into play continually, giving us most inconstant results. Yet there is no breach of determinism here; the determining feature lies both for the machine and for the organism in the arrangement of parts.

In view of these facts, it is not surprising that we often get nothing fundamental by determining the action of a given chemical or physical agent on living substance. It is often assumed, tacitly or openly, that after such a determination is made its results can be transferred to other masses of living material; can be generalized. But the effects seen are usually due to the characteristic arrangement of the material acted upon; they disappear or are reversed as soon as we work with material in other arrangements. We cannot therefore expect the study of the direct simple action of chemical and physical agents to help us greatly in understanding why an animal does what it does, though this has been heralded as the one right way to study behavior. From the action of the same agent most diverse results follow, depending on the arrangement of the material on which it acts.

Second, we find, both in machines and in organisms, that a mass of substance may respond in the same way to reagents of the most diverse kind, reagents having varied and even opposite effects. The avoiding reaction in *Paramecium* may be caused by heat and by cold; by acids, by alkalies, and by salts; by electrolytes and by non-electrolytes; by increase of osmotic pressure and by decrease of osmotic pressure; by chemical action without change of osmotic pressure; by mechanical shock and by electric shock; and this is a type of what we find in organisms. How often of late in the history of comparative physiology have we seen a certain effect attributed to one specific physico-chemical principle after another,—always on the assumption that the direct physical action of the agent is the essential point. Increase in osmotic pressure is first heralded as the essential point, until we find that decrease in osmotic pressure has the same effect, and that chemical change without change in osmotic pressure has likewise this effect. Then positively charged ions are made responsible, till we find that negatively charged ions will produce the same effect. And so the gamut is run; till heat, cold, mechanical shock, and the most diverse agents are found acting in the same way.¹

Parallel results are easily obtained from combinations of inorganic material. Imagine an arrangement such that when an electric connection is made, some characteristic action is per-

¹For an example lying outside the field of behavior, the history of the theories of artificial parthenogenesis is most instructive.

formed; for example, a bell is rung. It is easy to so arrange that reagents of opposite character shall make the connection and so ring the bell. Thus the electric button could be placed on a support between and close to an upper and a lower post, contact with either of which completes the circuit. Now, heating the support would lengthen it and cause contact with the upper post, while cold would shorten it and cause contact with the lower post; in either case the bell would ring. Similarly, it would be easy to so arrange a circuit that addition of acid or alkali or any electrolyte would close it; or even so that non-electrolytes would have the same effect. By having a somewhat complex structure, the most varied reagents would all close the circuit and ring the bell. This is the sort of thing we find in organisms. From diverse reagents we may get the same reaction; from the same reagent we get sometimes one result, sometimes another.

It is this condition of affairs that has brought upon the head of the investigators that report it accusations from the narrower physico-chemists of indeterminism, of vitalism, and from the vitalists claims that physico-chemical explanations have failed.

Yet we find the same relations in the inorganic combinations that we call machines. In machines do the physico-chemical properties of the material determine its action? Is it proper to study these physical and chemical properties in order to understand how the machine acts?

Evidently these questions are to be answered *yes!* Only, we must study these properties intelligently, recognizing the fact that the properties due to a certain arrangement of material are among the most important of all, for it is to this arrangement that the specific action of the machine or the organism is due. From a mere study of the properties of unarranged iron, ivory, paint, ink, and of the action of various agents upon them, we could never understand the typewriter. So from a mere study of the unarranged material of organisms and the action of agents upon them, we can never hope to understand their behavior.

Herein lies the failure or inadequacy of much of the physico-chemical work along these lines; it has dealt mainly with the properties of unarranged material, when the arrangement is precisely the essential point. There is among certain general physiologists an intense prejudice against all morphology; a cardinal point of faith is that structure is of no account. By taking this stand, they play directly into the hands of the vitalists. Vitalistic theories flourish as a result of too simple statements of the problems, and too simple solutions, on the part of the physico-chemists. If we maintain that physico-chemical explanation means that behavior in

lower organisms is the direct result of simple chemical and physical action on a certain kind of substance,—then such explanation undoubtedly fails, and the vitalist triumphs.

What is most needed is that the physico-chemical student of biology shall realize that as matter takes on new arrangements, its activities and reactions become different, even though the properties of each constituent part may remain the same. Since in living things there are beyond doubt arrangements differing from anything found elsewhere, we are of course certain to find in living things ways of acting that differ from anything found elsewhere. Hence we cannot expect to find in the physics and chemistry of inorganic matter the full explanation of the activities of organisms; those who expect to do this are following a will-o'-the-wisp,—and this is certain from physico-chemical principles themselves. The physicist and chemist must study organisms in order to fully understand physics and chemistry, just as the biologist must study physics and chemistry in order to understand organisms.¹

The arrangement of the material is then the essential point in determining behavior. What then becomes from the mechanistic standpoint the fundamental problem of behavior? Is it not the question of how these arrangements of material arise, and how they become changed? This property of taking on typical arrangements and of changing these arrangements is the fundamental and essential property of living matter that requires study for the physico-chemical understanding of behavior.

Of such study hardly a beginning has been made. With regard to the minute internal physical changes in the taking on of new arrangements, we have merely some faint suggestions from the rearrangement of particles in the hysteresis of

¹Some writers have applied the name vitalism to the idea that new methods of action arise when new combinations occur, taken in connection with the view that new combinations are found in living things; so apparently Radl (*Geschichte Biologischer Theorien*, vol. 1, p. 81). But such vitalism involves no new principle of explanation; it is based upon conditions found in chemistry and physics as decidedly as in biology. New methods of action arise when oxygen and hydrogen combine, producing water; new methods of action arise when a mass of brass and iron is arranged in the form of a clock. How then can it fail to be true in the case of organisms? The study of physics and chemistry is the study of the methods of action of matter and energy, whether simple or in combination. No new principle of explanation is involved simply because the combinations studied are those in living things. This view seems therefore to the writer far from a vitalistic one. On this point, see the excellent discussion by O. Hertwig, *Der Kampf um Kernfragen der Entwicklungs- und Vererbungslehre* (Jena, 1909), pp. 55-81; notably p. 80.

colloids. A little more has been done in tracing the external manifestations of these changes in the individual organism, through the study of changes of behavior in different physiological conditions, and in the formation of habits and associations. The most important part of the problem lies in the question of how diversities of arrangement arise during the racial history.

It is here that the problem of behavior opens into the more general problems of heredity, variation and evolution. This is the field that underlies the study of behavior. How does the living substance become modified in the lapse of time, so as to take on new arrangements, and therefore to behave differently? On this, if we stick to our ideal of demanding to observe the processes as they occur, little has been accomplished. Von Uexküll, with this ideal in view, feels that a correct statement is this: that all we know about this process is that it does not occur as Darwin thought it did.¹ This is the central physico-chemical problem for a causal understanding of behavior. Compared with it, the study in living things of the changes in surface tension, osmotic pressure, the nature of solution, and the like, though important in themselves, can give us little help, save as they may underlie and finally lead to a knowledge of the properties and laws in accordance with which living matter takes on and changes its method of arrangement. This central problem of behavior is likewise the central problem of all biology.

Thus the condition of affairs we have sketched does not lead to indeterminism; to belief in the failure of physico-chemical explanations; to vitalism. It merely leads us to perceive that the problem is more complex than has been supposed; that the fundamental question is that regarding the production of arrangements in the living substance, and that the solution of this problem lies in the future.²

We have spoken of vitalism. What we have thus far set forth bears upon the problems of anthropomorphism, and of finalism or teleology, almost as directly as it does upon vitalism. We need therefore to touch upon these but briefly.

2. Anthropomorphism signifies unjustifiably reading into lower organisms the characteristics, particularly the mental

¹Von Uexküll: Studien über den Tonus, V. Zeitschr. f. Biol., vol. 50, p. 168.

²I need not say that this discussion is not presented as a "refutation" of any special brand of vitalism, for example, that of Driesch. For such a refutation the precise arguments set forth in support of vitalism would have to be taken up and overcome. I have here tried merely to set forth the attitude I have reached after some years of work on behavior.

ones, that we find in ourselves, and especially it signifies substituting these for a causal explanation. This is a serious error. But it has nothing to do with another question, with which it is often confused. This other question is, whether the behavior of animals resembles in any features the behavior of man. This is purely a question of objective fact, not one for prejudice or for *a priori* considerations of any sort. The only way to answer it is to learn the objective facts for man and for animals, and then to compare them, observing where there are resemblances, where differences. To see such an objective comparison proposed seems to arouse all the fighting instincts of some of our ultra physico-chemical friends. just as it does some of our theological friends. But if we persist in making it undisturbed, we undoubtedly find many fundamental resemblances, along with many differences, between the behavior of even the lowest organisms and of man. Some of these are: the fact that reactions are due to the release of internal energy; that action may occur without specific external stimulus; that action is modified by internal changes of the most varied character, many of which are parallel in man and protozoan; that negative reactions are given mainly to injurious agents, positive ones to beneficial agents; that varied reactions occur under the influence of a single constant stimulus, and that the organism tends to persist in that reaction which keeps it in conditions favorable to its life processes.

To point out these and similar resemblances is merely to point out the facts, with which we must all come to some sort of a working compromise. The existence of organisms whose behavior has these characteristics is not inconsistent with physico-chemical explanation, if physico-chemical explanation is valid, for man does exist. No question of fundamental principle is then involved when we find that other organisms have these characteristics; the question is merely as to the distribution of characteristics known to exist.

The problem of behavior then is : How are such characteristics to be explained from the physico-chemical point of view? If the practical physico-chemist declares that they cannot ultimately be so explained, he merely condemns his own methods of work and places himself among the vitalists. For such faint-heartedness there is surely as yet no justification! We have indeed to attack certain problems hardly yet approached,—the study of the physico-chemical properties in virtue of which certain materials become arranged in such characteristic ways as to produce the phenomena we have mentioned.

3. The matter of teleology or finalism is in similar case. We find as a mere matter of fact, certain marked relations be-

tween a present process and something that exists later. These relations resemble in many ways the relations between action that in man is accompanied by a purpose, and the result of that action. We find such relations in many fields besides behavior; the physiologist is compelled to recognize them everywhere. In the study of behavior it has become a sort of popular fad to ignore these relations; to act as if they did not exist. These are merely the tactics of the ostrich. We must face the problems which nature sets us. When the lens of the eye develops in the dark, it is mere cowardice to try to act as if we did not know that this lens is so constituted as to bring light to a focus. When the pancreas secretes an enzyme, it is again struthionic tactics to refuse to see the relation of that enzyme to the digestion of food,—although this digestion does not take place till after the secretion has occurred. Behavior in lower organisms is an almost continuous tissue of such relations; they are largely what we call the regulatory features of behavior.

When the ultra physico-chemist assumes that the pointing out of these extraordinary relations is given as an explanation of them, he shows most surprising naïveté. They form precisely the most difficult, the most complex, problem for causal explanation. The question is; How were such relations brought about? This is a question for precisely the same kind of objective investigation as the question how any other relation was brought about. How does the boulder happen to lie in the middle of the plain? How does the lens get into the optic cup? How does the lens happen to be of such a form as to bring light to a focus? How does the animal happen to react in such a way as to protect itself? The answer in every case lies in tracing the processes by which the relations were brought about, and in discovering the laws of these processes; by beginning with the condition where these relations do not exist, and tracing the "particular go" of the changes until these relations do exist.

How the relations that impress us as teleological were brought about constitutes undoubtedly a set of most difficult problems. But to keep us from despairing, we find this process taking place in the lives of individuals in a manner that can readily be studied. This is in the formation of habits. In the formation of habits, we see that the organism at first does not react in a way that impresses us as teleological, while later it does, and we can watch the process of change from one condition to the other, and discover how it is causally determined. Since then a method of action that appears to us teleological is produced in an intelligible way under our very eyes, in the lifetime of the individual, there is no reason why we

may not expect to find out how teleological relations have been brought about in the life of the race, when we have actually made a start in the study of the physiology of racial processes.¹ It seems clear that the apparent relation of a present process or structure to something that comes later in time is always due to the fact that this future something has in fact acted upon the organism in the past. The present condition fits the future condition only because of a certain constancy in the universe, through which the 'something past' reappears again in the future.

Let us then attempt in closing to characterize behavior as we find it in its lowest stages, recapitulating the chief points we have made. We find in lower organisms, as in higher animals, that the nature of the reactions is due mainly to characteristic *arrangements* of material, not to the properties of simple unarranged substance. These lower organisms therefore furnish problems which do not differ in kind from what we find in higher animals. They are simpler only in a numerical sense, — in that their parts are less numerous than those of higher animals. It would be most interesting if we found in these lower creatures a half way stage to inorganic matter; if we found living matter without characteristic arrangements, so that its properties and actions were those of an undifferentiated substance. But perhaps the chief result of research is to show that we do *not* find this condition realized. Doubtless this is a great disappointment, much diminishing the supposed importance of studying the lowest organisms. But we must bow to the facts.

In the behavior of these lower creatures we do not find that uniformity which certain physico-chemical theories of behavior demand. With certain underlying conditions in common, extreme diversities in methods of action are the rule. To the same stimuli different organisms react differently; different individuals of the same species react differently, and even the same individual reacts differently at different times. As Walter has well said "strictly speaking, all behavior is individual behavior,"² and Driesch³ has shown his usual acumen in setting

¹ The statement is sometimes made that we can never hope to reduce teleological relations to causal relations (see v. Uexküll, *Leitfaden in das Studium der experimentellen Biologie der Wassertiere*, 1905, p. 129). This doubtless means that the teleological relations remain, even after the process by which they are brought about has been explained causally. It certainly cannot be maintained, in view of the known process of forming useful habits, that the process itself cannot be understood from a causal standpoint.

² Walter, H. E.: *The Reactions of Planarians to Light*. *Journ. Exper. Zool.*, 5, 1907, p. 97.

³ Driesch, H.: *The Science and Philosophy of the Organism*, 1908-9.

forth individuality as the central problem of biology. For the purposes of physico-chemical explanation, individuality in behavior is an outgrowth of the fact that there is practically infinite diversity in the arrangement of living substance, so that no two specimens of it are precisely alike, therefore they do not act precisely alike. Behavior in lower organisms presents itself exactly as it would if the theory of descent with unlimited modifiability were true,—every individual forming a centre from which modifications may and do diverge in many directions. If we abandon for a moment our requirement of seeing evolution occur before we accept it, and assume that it *has* occurred, then we should hold that lower organisms are not really more primitive than higher ones; each may have as long a history and as many modifications behind it as a higher animal. They would be conceived merely as creatures that have retained certain numerically simple arrangements, because they thus fit some otherwise unoccupied nooks and crannies of the universe.

The great problem of behavior then, as for biology in general, is to work out those properties of living matter and of the environment, by which characteristic arrangements of material are produced and modified. In all these typical arrangements or structures that we call organisms, including the protozoan and man, there are certain common features in behavior; the pointing out of these common features is at times denounced as anthropomorphism. Again, in the behavior of these typical arrangements of material, we observe certain relations of present actions to later conditions; these relations we call teleological. But how these conditions and relations are brought about is essentially a physico-chemical problem, in the sense that we can study only the processes and configurations of matter and energy from which they result. The entire actual situation in behavior is that on which theories of vitalism have been based. It can truly be said that the condition of affairs found in behavior is very nearly that set forth by the vitalists as a basis for vitalism; their conception of the gross facts of behavior is more nearly accurate and adequate, from a descriptive standpoint, than is that set forth by the simplifying physico-chemists. The vitalists take into consideration all the phenomena, while the ultra physico-chemists leave out of account the most interesting ones.

But this does not mean that vitalism is anything more than a name for what we have not yet worked out; it does not mean the giving up of the ideal of essentially physico-chemical or mechanistic explanation. It means merely that we must recognize the enormous complexity and difficulty of the physico-

chemical problem ;¹ must realize that we have hardly gotten hold of the first threads for unravelling the puzzle yet; that we have, indeed, hardly attacked the real problem, save in the mass and by analysis into components that are themselves inconceivably complex. The central problem, of working out the laws and processes by which typical arrangements of matter and energy are produced and modified in organisms, presents itself as a problem to be attacked only by the essentially physico-chemical methods through which all the real causal explanation that we have has thus far been reached.

¹ "Nature cannot be made simple by treating her on the theory that she ought to be so, when as a matter of fact she is not." Ritter, l. c., p. 187.

PSYCHOLOGICAL PROBLEMS IN ANTHROPOLOGY¹

By FRANZ BOAS

The science of anthropology deals with the biological and mental manifestations of human life as they appear in different races and in different societies. The phenomena with which we are dealing are therefore, from one point of view, historical. We are endeavoring to elucidate the events which have led to the formation of human types, past and present, and which have determined the course of cultural development of any given group of men. From another point of view the same phenomena are the objects of biological and psychological investigations. We are endeavoring to ascertain what are the laws of hereditary stability and of environmental variability of the human body. These may be recognized in the historical changes that the bodily appearance of man has undergone in the course of time, and in his displacement from one geographical or social environment to another. We are also trying to determine the psychological laws which control the mind of man everywhere, and that may differ in various racial and social groups. In so far as our inquiries relate to the last-named subject, their problems are problems of psychology, though based upon anthropological material. I intend to speak of this aspect of anthropology to-day.

The fundamental problem on which all anthropological inquiry must be founded relates to the mental equipment of the various races of man. Are all the races of mankind mentally equally endowed, or do material differences exist? The final answer to this question has not been given, but anatomical observations on the various races suggest that differences in the form of the nervous system are presumably accompanied by differences in function, or, psychologically speaking, that the mental traits which characterize different individuals are distributed in varying manner among different races; so that the composite picture of the mental characteristics of one race would presumably not coincide with the composite picture of the mental characteristics of another race. The evidence that has been brought forward does not justify us,

¹Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

however, in claiming that the characteristics of one race would be an advance over those of another, although they would be different.

This question has also been approached from the standpoint of racial achievement. It has been pointed out that only the white race and the Mongolian race have reached any high grade of cultural development, and on this basis it has been assumed that the other races of man have not the ability to reach the same grade of civilization. It has been shown, however, that the retardation of the other races is not necessarily significant, because the amount of retardation is small as compared to the time consumed in reaching the present stages. It would seem, therefore, that the weight of evidence is, on the whole, in favor of an essential similarity of mental endowment in different races, with the probability of variations in the type of mental characteristics. Further inquiries into this subject must be based not only on sociological studies, but also on anatomical, physiological, and psychological inquiries among individuals belonging to the distinct races of mankind.

While the problem that I have just outlined relates to hereditary racial differences, a second fundamental problem of anthropology relates to the mental characteristics of social groups regardless of their racial descent. Even a superficial observation demonstrates that groups of man belonging to distinct social strata do not behave in the same manner. The Russian peasant does not react to his sense experiences in the same way as does the native Australian; and entirely different from theirs are the reactions of the educated Chinaman and of the educated American. In all these cases the form of reaction may depend to a slight extent upon hereditary individual and racial ability, but it will to a much greater extent be determined by the habitual reactions of the society to which the individual in question belongs.

The reaction of a member of a society to the outer world may be twofold. He may act as a member of a crowd, in which case his activities are immediately determined by imitation of the activities of his fellows; or he may act as an individual; then the influence of the society of which he is a member will make itself felt by the habits of action and thought of the individual.

I have discussed the racial question repeatedly at other places. The problem of the psychology of the crowd is a peculiarly intricate one, based largely upon the data of social psychology in a wider sense of the term, and upon data of individual psychology. I may be allowed for these reasons to confine myself to-day to the third of the problems which I have

outlined, that of the psychological laws which govern man as an individual member of society.

This problem has been the object of intensive study by the great minds that have laid the foundation of modern anthropology. The ultimate aim of Waitz's great work is the inquiry into the question whether there are any fundamental differences between the mental make-up of mankind the world over, racially as well as socially. Tylor, in his brilliant investigations on the development of civilization, showed the common occurrence of similar types of ideas the world over, and demonstrated the possibility of conceiving of the scattered phenomena as proof of certain tendencies of evolution of civilization. The many investigators who have studied the evolution of marriage relations, the evolution of law, of art, of religion, all start from the same basis—the assumption of a general similarity of mental reaction in societies of similar structure. Bastian has tried to prove by the use of anthropological data that man the world over develops the same elementary ideas, on which the fabric of his mental activities is based; and that these elementary ideas may be modified by geographical and social environment, but that they remain essentially the same everywhere.

It may be well to illustrate the facts here referred to by a few examples. In the domain of industrial activity we find that mankind is everywhere in possession of the art of producing fire by friction, that everywhere food is prepared by cooking, that shelters are built, that tools are used for breaking and cutting. We do not know mankind in any stage where any of these inventions are absent. In regard to social structure we find that man nowhere lives alone; that even the cases in which the social group consists of members of one family only, are exceedingly rare and of temporary occurrence. We furthermore find that the social units are subdivided into groups, which are kept apart by customary laws forbidding intermarriages in one group, and prescribing intermarriages in another.

In the domain of religion an idea of this type is that of life after death. There is probably no people that believes in the complete extinction of existence with death, but some belief in the continuity of life seems to exist everywhere. To the same domain belongs that type of concepts of the world, in which the surface of our earth is considered as forming a central level, above and below which other worlds are located.

An examination of the types of ideas represented by the few examples that I have here given shows that their subject-matter is highly complex, and that in a strict sense the occurrence of these ideas by itself does not explain clearly the psychological

processes that produce them and that cause their stability. Attempts at a psychological interpretation of these concepts have often been made by means of a comparative treatment of similar ideas, and by endeavors to arrange these ideas in such a way as to show a more or less rationalistic development of one from the other. While this may be feasible in some cases, it does not seem likely that this method of treatment will lead us to the most generalized laws governing the forms of thought in human societies.

The principal obstacle in the way of progress on these lines seems to my mind to be founded on the lack of comparability of the data with which we are dealing. When, for instance, we speak of the idea of life after death as one of the ideas which develop in human society as a psychological necessity, we are dealing with a most complex group of data. One people believes that the soul continues to exist in the form that the person had at the time of death, without any possibility of change; another one believes that the soul will be reborn in a child of the same family; a third one believes that the souls will enter the bodies of animals; and still others that the shadows continue our human pursuits, waiting to be led back to our world in a distant future. The emotional and rationalistic elements which enter into these various concepts are entirely distinct; and we can readily perceive how the various forms of the idea of a future life may have come into existence by psychological processes that are not at all comparable. If I may be allowed to speculate on this question, I might imagine that in one case the similarities between children and their deceased relatives, in other cases the memory of the deceased as he lived during the last days of his life, in still other cases the longing for the beloved child or parent, and again the fear of death—may all have contributed to the development of the idea of life after death, the one here, the other there.

Another instance will corroborate this point of view. One of the striking forms of social organization, which occurs in many regions wide apart, is what we call totemism,—a form of society in which certain social groups consider themselves as related in a supernatural way to a certain species of animals or to a certain class of objects. I believe this is the generally accepted definition of totemism; but I am convinced that in this form the phenomenon is not a single psychological problem, but embraces the most diverse psychological elements. In some cases the people believe themselves to be descendants of the animal whose protection they enjoy. In other cases an animal or some other object may have appeared to an ancestor of the social group, and may have promised to become his protector, and the friendship between the animal and the ancestor

was then transmitted to his descendants. In still other cases a certain social group in a tribe may have the power of securing by magical means and with great ease a certain kind of animal or of increasing its numbers, and the supernatural relation may be established in this way. It will be recognized that here again the anthropological phenomena, which are in outward appearances alike, are, psychologically speaking, entirely distinct, and that consequently psychological laws covering all of them can not be deduced from them.

Another example may not be amiss. In a general review of moral standards we observe, that, with increasing civilization, a gradual change in the valuation of actions takes place. Among primitive man human life has little value, and is sacrificed on the slightest provocation. The social group among whose members any altruistic obligations are binding is exceedingly small; and outside of the group any action that may result in personal gain is not only permitted, but even approved; and from this starting point we find an ever-increasing valuation of human life and an extension of the size of the group among whose members altruistic obligations are binding. The modern relations of nations show that this evolution has not yet reached its final stage. It might seem, therefore, that a study of the social conscience in relation to crimes like murder might be of psychological value, and lead to important results, clearing up the origin of ethical values; but I think here the same objections may be raised as before, namely the lack of comparable motives. The person who slays an enemy in revenge for wrongs done, a youth who kills his father before he gets decrepit in order to enable him to continue a vigorous life in the world to come, a father who kills his child as a sacrifice for the welfare of his people, act from such entirely different motives, that psychologically a comparison of their activities does not seem permissible. It would seem much more proper to compare the murder of an enemy in revenge, with destruction of his property for the same purpose, or to compare the sacrifice of a child on behalf of the tribe with any other action performed on account of strong altruistic motives, than to base our comparison on the common concept of murder.

Similar observations may also be made in the domain of art. The artist who tries to display his skill in handling his material will be led to æsthetic results. Another one, who wishes to imitate certain forms in his work, may be led to similar results. Notwithstanding similarity of results, the psychological processes in these two cases are quite distinct and not comparable.

For these reasons it seems to me that one of the fundamental points to be borne in mind in the development of anthropological psychology is the necessity of looking for the

common psychological features, not in the outward similarities of ethnic phenomena, but in the similarity of psychological processes so far as these can be observed or inferred.

Let us next consider in what direction the psychological problems of anthropology have to be looked for. I must confine myself here to a very few examples of what seem to me fundamental psychological facts.

One of the most striking features in the thoughts of primitive people is the peculiar manner in which concepts that appear to us alike and related are separated and re-arranged. According to our views the constituting elements of the heavens and of the weather are all inanimate objects; but to the mind of primitive man they appear to belong to the organic world. The dividing-line between man and animal is not sharply drawn. What seem to us conditions of an object—like health and sickness—are considered by him as independent realities. In short, the whole classification of experience among mankind living in different forms of society follows entirely distinct lines. I believe this subject can be made clear most easily by a comparison with a similar phenomenon in languages.

If the whole mass of concepts, with all their variants, were expressed in language by entirely heterogeneous and unrelated sound complexes, a condition would arise in which closely related ideas would not show their relationship by the corresponding relationship of their phonetic signs. An infinitely large number of distinct sound complexes—in other words, of distinct words—would be required for expression. If this were the case, the association between an idea and its representative sound complex would not become sufficiently stable to be reproduced automatically at any given moment, without reflecting. The automatic and rapid use of language has brought it about that the infinitely large number of ideas have been reduced by classification to a lesser number, which by constant use have established firm associations, and which can be used automatically. It seems important to emphasize the fact that the groups of ideas expressed by specific words show very material differences in different languages, and do not conform by any means to the same principles of classification. To take the example of English. We find that the idea of water is expressed in a great variety of forms. One term serves to express water as a liquid; another one, water in the form of a large expanse, a lake; others, water as running in a large body or in a small body, a river and brook. Still other terms express water in the forms of rain, dew, wave, and foam. It is perfectly conceivable that this variety of ideas, each of which is expressed by a single independent term in English, might be expressed in other lan-

guages by derivations from the same term. It seems fairly evident that the selection of simple terms must to a certain extent depend upon the chief interests of a people; and where it is necessary to distinguish a certain phenomenon in many varieties, which in the life of a people play each an entirely independent rôle, many independent words may develop, while in other cases modifications of a single term may suffice. In the same way as concepts are classified and groups of perceptions are expressed by a single term, relations between perceptions are also classified. The behavior of primitive man makes it perfectly clear that all these linguistic classes have never risen into consciousness, and that consequently their origin must be sought not in rational, but in entirely unconscious processes of the mind. They must be due to a grouping of sense impressions and of concepts which is not in any sense of the term voluntary, but which develops from entirely different psychological causes. It is a characteristic of linguistic classifications that they never rise into consciousness, while other classifications, although the same unconscious origin prevails, often do rise into consciousness. It seems very plausible, for instance, that the fundamental religious notions, like the idea of will power immanent in inanimate objects, or the anthropomorphic character of animals, are in their origin just as little conscious as the fundamental ideas of language. While, however, the use of language is so automatic that the opportunity never arises for the fundamental notions to emerge into consciousness, this happens very frequently in all phenomena relating to religion.

I believe that anthropological investigations carried on from this point of view offer a fruitful field of inquiry. The primary object of these researches would be the determination of the fundamental categories under which phenomena are classified by man in various stages of culture. Differences of this kind appear very clearly in the domain of certain simple sense-perceptions. For instance, it has been observed that colors are classified according to their similarities in quite distinct groups without any accompanying difference in the ability to differentiate shades of color. What we call green and blue are often combined under some such term as "gall-like color," or yellow and green are combined into one concept, which may be named "young-leaves color." The importance of the fact that in thought and in speech these color-names convey the impression of quite different groups of sensations can hardly be over-rated.

Another group of categories that promise a field of fruitful investigation are those of object and attribute. The concepts of primitive man make it quite clear that the classes of ideas

which we consider as attributes are often considered as independent objects. The best-known case of this kind, one to which I have referred incidentally before, is that of sickness. While we consider sickness as a condition of an organism, it is believed by primitive man, and even by many members of our own society, to be an object which may enter the body, and which may be removed. This is exemplified by the numerous cases in which a disease is extracted from the body by sucking or by other processes, in the belief that it may be thrown into people, or that it may be enclosed in wood in order to prevent its return. Other qualities are treated in the same way. Thus the condition of hunger, exhaustion, and similar bodily feelings, are considered by certain primitive tribes as independent objects which affect the body. Even life is believed to be a material object that may become separated from the body. The luminosity of the sun is considered as an object that the Sun himself may put on or lay aside.

I have indicated before that the concept of anthropomorphism seems to be one of the important categories underlying primitive thought. It would seem that the power of motion of the self and the power of motion of an object have led to the inclusion of man and movable objects in the same category, with the consequent imputation of human qualities to the moving objective world.

While in many cases we can see with a fair degree of clearness the fundamental concepts underlying these categories, in other cases these are not by any means clear. Thus the concept of incest groups—those groups in which intermarriage is strictly forbidden—is omnipresent. But no satisfactory explanation has so far been given for the tendency to combine certain degrees of blood relationship under this view-point.

Much material for this field of inquiry is contained in the works on comparative anthropology, but I believe a more thorough psychological analysis of the accumulated data may reveal important new information.

We will now turn to the consideration of another group of psychological phenomena that seem to me of considerable importance. In all forms of society certain groups of activities and of thoughts appear in certain typical associations. Thus in our modern society the consideration of cosmic phenomena is constantly associated with the efforts to give adequate explanations for them, based on the principle of causality. In primitive society the consideration of the same phenomena leads to a number of typical associations which differ from our own, but which occur with remarkable regularity among tribes living in the most remote parts of the world. An excellent instance of this kind is the regular association of obser-

vations relating to cosmic phenomena with purely human happenings; in other words, the occurrence of nature myths. It seems to my mind that the characteristic trait of nature myths is the association between the observed cosmic events and what might be called a novelistic plot based on the form of social life with which people are familiar. The plot as such might as well develop among the peoples themselves; but its association with the heavenly bodies, the thunder-storm, or the wind, makes it a nature myth. The distinction between the folk-tale and the nature myth lies solely in the association of the latter with cosmic phenomena. This association does not naturally develop in modern society. If it is still found every now and then, it is based on the survival of the traditional nature myth. In primitive society, on the other hand, it is found constantly. The investigation of the reason for this association is an attractive problem, the solution of which can only in part be surmised.

A number of other examples will demonstrate that the kind of association here referred to is quite common in primitive life. An excellent instance is furnished by certain characteristics of primitive decorative art. With us almost the sole object of decorative art is æsthetic. We wish to beautify the objects that are decorated. We recognize a certain appropriateness of decorative motives in accordance with the uses to which objects are to be put, and the emotional effect of the decorative motive. In primitive life the conditions are quite different. Extended investigations on decorative art in all continents have proved that practically everywhere the decorative design is associated with a certain symbolic significance. There is hardly a case known where a primitive tribe cannot give some sort of explanation for the designs they use. In some cases the symbolic significance may be exceedingly weak, but ordinarily it is highly developed. The triangular and quadrangular designs of our Plains Indians, for instance, almost always convey definite symbolic meanings. They may be records of warlike deeds, they may be prayers, or they may in some way convey other ideas relating to the supernatural. It would almost seem that among primitive tribes decorative art for its own sake does not exist. The only analogies in modern decorative art are such as the use of the flag, of the cross, or of emblems of secret societies, for decorative purposes; but their frequency is insignificant as compared to the general symbolic tendencies of primitive art. Thus it will be seen that we have here again a type of association in primitive society quite different from the type of association found among ourselves. Among primitive people the æsthetic motive is combined with the symbolic,

while in modern life the æsthetic motive is either quite independent, or associated with utilitarian ideas.

I will give still another example of a form of association characteristic of primitive society. In modern society, social organization, including the grouping of families, is essentially based on blood relationship and on the social functions performed by each individual. Except in so far as the Church concerns itself with birth, marriage, and death, there is no connection between social organization and religious belief. These conditions are quite different in primitive society, where we find an inextricable association of ideas and customs relating to society and to religion. I have referred before to the phenomena of totemism, which are perhaps the best example of this type of association. Totemism is found among many American tribes, as well as in Australia, Melanesia, and in Africa. I have described before its characteristic trait, which consists in supernatural connection that is believed to exist between a certain class of objects, generally animals, and a certain social group. Further analysis shows very clearly that one of the underlying ideas of totemism is the existence of definite groups of man that are not allowed to intermarry, and that the limitations of these groups are determined by considerations of blood relationship. The religious ideas found in totemism refer to the personal relation of man to certain classes of supernatural powers, and the typical trait of totemism is the association of certain kinds of supernatural power with certain social groups. Psychologically, therefore, we may compare totemism with those familiar forms of society in which certain social classes claim privileges by the grace of God, or where the patron saint of a community favors its members with his protection. It will be recognized that we have here again a type of association in primitive society which has completely changed with the development of civilization.

We will now turn to the consideration of a third point, to the peculiar importance of automatic actions in the development of the customs and beliefs of mankind. It is a well-known fact that all those actions which we perform with great frequency are liable to become automatic; that is to say, that their performance is ordinarily not combined with any degree of consciousness. Consequently the emotional value of these actions is also very slight. It is, however, remarkable that the more automatic an action, the more difficult it is to perform the opposite action; that it requires a very strong effort to do so; and that ordinarily the opposite action is accompanied by strong feelings of displeasure. It may also be observed that to see the unusual action performed by another person excites the strongest attention and causes feelings

of displeasure. An example will make clear what I mean. When we consider our table manners, it will readily be recognized that most of them are purely traditional and cannot be given any adequate explanation. Still the constant performance of the actions which constitute good table manners makes it practically impossible for us to act otherwise. An attempt to act differently would not only be difficult on account of the lack of adjustment of muscular motions, but also on account of the strong emotional resistance that we should have to overcome. To eat with people having table manners different from our own seems to us decidedly objectionable and causes feelings of displeasure which may rise to such intensity as to cause qualms. Another good example is the feeling connected with acts that in our society are considered as modest or immodest. Every one will feel instinctively the strong resistance that he would have to overcome, even in a different society, if he were required to perform an action that we are accustomed to consider as immodest, and the feelings that would be excited in his mind if he were thrown into a society in which the standards of modesty differed from our own. It seems to my mind that these feelings of displeasure exert a very strong influence upon the development and conservation of customs. The young child in whom the habitual behavior of his surroundings has not yet developed will acquire much of this behavior by unconscious imitation. In many cases, however, it will act in a way different from the customary behavior, and will be corrected by its elders. This is presumably one of the most important elements that tend to bring customary behavior into the consciousness of the people practising it. When educating their children to conform to the tribal standards, these standards must necessarily become conscious to the educators.

One of the cases in which the development of ideas based on behavior is best traced, is that of the taboo. Although we ourselves have hardly any definite taboos, to an outsider our failure to use certain animals for food might easily appear from this point of view. Supposing an individual accustomed to eating dogs should inquire among us for the reason why we do not eat dogs, we could only reply that it is not customary; and he would be justified in saying that dogs are tabooed among us, just as much as we are justified in speaking of taboos among primitive people. There are a number of cases in which it is at least conceivable that the older customs of a people, under a new surrounding, develop into taboos. I think, for instance, that it is very likely that the Eskimo taboo forbidding the use of caribou and of seal on the same day may be due to the alternating inland and coast life of the people. When they hunt inland, they have no seals, and consequently can eat only

caribou. When they hunt on the coast, they have no caribou, and consequently can eat only seal. The simple fact that in one season only caribou can be eaten, and that in another season only seal can be eaten, may have easily led to a resistance to a change of this custom; so that from the simple fact that for a long period the two kinds of meat could not be eaten at the same time developed the law that the two kinds of meat must not be eaten at the same time. I think it is also likely that the fish taboo of some of our Southwestern tribes may be due to the fact that the tribes lived for a long time in a region where no fish was available, and that the impossibility of obtaining fish developed into the custom of not eating fish.

It would seem, therefore, that we may say in a general way that the customary action is the ethical action, that a breach of custom is everywhere considered as essentially unethical.

It is very likely that the same causes have had a strong influence upon the development of local conventional styles of art. It is no less true that the customary form is liable to be considered the beautiful form than that the customary behavior is considered ethical behavior. Therefore the stability of primitive styles of art may ultimately be due to the same causes as the stability of primitive customs.

If the origin of concepts and of distinct types of association is such as I suggested to-day, and if the existence of these concepts and types of association is brought into the consciousness of primitive man by the incidents of his daily life, when customary concepts and customary associations seem to be broken, we recognize that man must in a great many cases find himself confronted with the fact that certain ideas exist in his mind for which he cannot give any explanation except that they are there. The desire to understand one's own actions, and to get a clear insight into the secrets of the world, manifests itself at a very early time, and it is therefore not surprising that man in all stages of culture begins to speculate on the motives of his own actions.

As I have explained before, there can be no conscious motive for many of these, and for this reason the tendency develops to discover the motives that may determine our customary behavior. This is the reason why in all stages of culture customary actions are made the subject of secondary explanations that have nothing to do with their historical origin, but which are inferences based upon the general knowledge possessed by the people. I think the existence of such secondary interpretations of customary actions is one of the most important anthropological phenomena, and one which is hardly less common in our own society than in more primitive societies. It is a common observation that we desire or act

first, and then try to justify our desires and our actions. When, on account of our early bringing up, we act with a certain political party, most of us are not prompted by a clear conviction of the justice of the principles of our party, but we do so because we have been taught to respect it as the right party to which to belong. Then only do we justify our standpoint by trying to convince ourselves that these principles are the correct ones. Without reasoning of this kind, the stability and geographical distribution of political parties as well as of church denominations would be entirely unintelligible. A candid examination of our own minds convinces us that the average man in by far the majority of cases does not determine his actions by reasoning, but that he first acts, and then justifies or explains his acts by such secondary considerations as are current among us.

That the same conditions prevail to even a greater extent among primitive people can easily be shown by a number of examples. It has been pointed out before that decorative art among primitive people is almost everywhere symbolic. This does not preclude the possibility of designs, and even of the whole style, of one region being borrowed by the people of another region. This has been the case, for instance, among the tribes of our Northwestern Plains, who have borrowed much of their art from their more southern neighbors; but they have not adopted together with it the symbolical interpretations given by their neighbors, but invented interpretations of their own. I imagine that this is the outcome of a mental process which set in when the designs were found pleasing, and, according to the general character of primitive thought, a symbolic interpretation was expected. This was then secondarily invented in accordance with the ideas current among the tribe.

The same observation may be made in primitive mythology. The same kind of tales are current over enormous areas, but the mythological use to which they are put is locally quite different. Thus an ordinary adventure relating to the exploits of some animal may sometimes be made use of to explain some of its peculiar characteristics. At other times it may be made use of to explain certain customs, or even the origin of certain constellations in the sky. There is not the slightest doubt in my mind that the tale as such is older than its mythological significance. The characteristic feature of the development of the nature myth is, first, that the tale has associated itself with attempts to explain cosmic conditions—this has been referred to before—and, secondly, that when primitive man became conscious of the cosmic problem, he ransacked the entire field of his knowledge until he happened

to find something that could be fitted to the problem in question giving an explanation satisfactory to his mind. While the classification of concepts, the types of association, and the resistance to change of automatic acts, developed unconsciously, many of the secondary explanations are due to conscious reasoning.

In the preceding remarks I have tried to point out a direction in which anthropological data may be used to good advantage by the psychologist; that from a psychological point of view, the starting-point of our investigations must not be looked for in anthropological phenomena that happen to be alike in outward appearance, but that in many cases diverse phenomena are based on similar psychic processes, and that these offer to the investigator a promising line of attack.

THE DYNAMIC INTERPRETATION OF DEMENTIA PRÆCOX¹

By ADOLF MEYER

Up to recent years the ambition of scientific medicine was to trace all morbid conditions to some kind of anatomical lesion. This inevitably left a large field in which there was 'no pathology as yet,' and therefore a suspicion of inevitable chaos. The trend of the last decade and the experience with biological serum-reactions and especially also the progress of psychiatry has, however, greatly strengthened a *functional* and *biological* view of the events in living beings, so that the work of *pathology* appears to us primarily as the *determination of causal chains or conditions with the accuracy of an experiment*, and the lesions then take their place among the simple facts or symptoms, according to the extent to which they can be understood in terms of dynamic developments, *i. e.*, of cause (or conditions) and effect.

This functional way of seeing things has the great advantage of allowing us to arrange the facts as we see them.

Most attempts at translation of the functional facts into neurological homologies leave out the actual amount and duration of the function, and the all-important laws of compatibility and incompatibility of sequences, and the discrimination between what are *moving factors* or more decorative staging or incidents. Turning to the functional and dynamic conceptions allows us to remain true to neurological conceptions where we are entitled to any and yet to take in the entire psychological setting without which the events would be difficult to grasp, artificial and devoid of the spark of life and interest to most physicians.

The dynamic conception which I shall present in this lecture is not *new* in one sense, but in another it *reconquers* and makes safe the ground of common-sense psychology abandoned by medicine during the struggle against superstitions and also under the influence of philosophical speculations, now become indispensable again if we wish to bring differentiation into our field and account for, and explain, the

¹Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

events constituting mental disorders. I have been said to resurrect the layman's opinion of the causes and nature of mental disease. I grant that to some extent; but we are also adding the *conditions* and critical safeguards under which we are justified in accepting and pursuing our normal instinctive interpretations.

Among mental diseases certain chains of events recur with such regularity that they become valuable clinical units or reaction-types.¹ When the development appears natural and plausible we consider the disorder accounted for and, under certain conditions, explained. In this respect, we find for instance that the so-called organic dementias are accounted for and to some extent explained by the extent and kind of brain disease, determination of which tells us what the symptoms and the development must have been or would probably be. In a more functional field, we find the deliria of intoxication, perhaps not explained, but empirically accounted for by a sufficient amount of intoxicant material *and* personal disposition. In a second place we may rank certain depressions and excitements and paranoic developments of which we may say that they come nearer and nearer being both explained and accounted for psycho-biologically; when we have all the facts, they are apt to rank fairly plainly as exaggerations of relatively normal reactions, as is the case with many depressions and delusional states. In a third place there are, however, disorders which seem to defy both explanation and accounting for; among these figure, according to the claims of many physicians, certain disorders prominent in the *dementia præcox* group. In order to be on concrete ground, I shall now briefly state a few of the types to be compared and discussed; and in view of the fact that we have with us the most fruitful workers on the problems of detail, I have chosen to limit myself especially to the broader settings in cases which anybody can find and study and form his own impressions and estimates on.

The following sketches form the material of our discussion :

A young man said to have been bright at school and in his early environment is transplanted from the South to New York at 22 (1903), fails to make friends, becomes morose and morbid and seclusive, especially after finally losing his position (Christmas, 1904); he resorts to quack-treatment for sexual neurasthenia, and finally goes to a general hospital with vague complaints of general weakness (Spring of 1905). Then, at home for six weeks, he showed *more* seclusiveness, was noticed to smile and talk to himself in an unexplained manner, imagined he was followed, in other words had the feeling of being at a disadvantage, and of mistrust and day-dream-like reac-

¹The *Psychological Bulletin*, Vol. V, No. 8, p. 245. The Problems of Mental Reaction-types, Mental Causes and Diseases.

tions, not clearly sized up by the family, until May 31, 1905, after a restless and probably sleepless night, a tantrum precipitated the recognition of the condition. The young man came to breakfast unkempt, ate, then suddenly got up, shook hands with his mother, said she was the best mother he ever had and announced: 'Between you and me and E (sister) I am going to be President of the United States.' The mother was (probably quite unnecessarily) frightened, he tried to hold her; she escaped and he then shouted and broke up the furniture and the chandelier. When he came to the hospital he regretted the violence, said he did not know at the time what he was doing; 'It seemed like a feeling took control of me.' He remembered it all clearly, and added that at times he felt as if *somebody* was taking something out of him when they spoke, 'some of my mind, my intellect, my power'—feelings and judgments very characteristic of this type of dissociation of the personality. At the hospital he was able to occupy himself for a while but gradually grew more irritable and offish, more and more dull and finally passed into a classical negativistic stupor (with catalepsy, refusal of food and mutism) from which he rose to some extent in the course of a few years, but with decided dilapidation, gross indifference, tendency to lounging about, with an occasional semblance of distractibility and flight of talk, but absence of any spontaneous push or interests—a classical instance of terminal dementia.

Heredity is denied in this case. Early masturbation stands in the centre of a distinct bias; circumstances and this bias encouraged solitude, hypochondriacal ruminations, feelings of disadvantage, neglect of himself, a shirking of the usual balancing helps offered by the common habits of social and practical life of the environment; then came more positively morbid reactions: mistrust, ideas of being followed, and silent mulling over thoughts never brought to any control by action, or even to simple open ventilation, as shown in the smiling and muttering to himself. Of this long period we only know the further frustration of interest in even the ordinary personal attentions and ambitions, and a peculiar notion that the doctor and nurses stole his soul (probably an imagination partly based on the feeling of disadvantage in comparisons of his empty state with the healthy, vigorous and superior persons with whom he had forced and otherwise dodged contact), and the episodes of probably very empty compensatory pleasurable moods of mere smiles, and mutterings of matters enjoyed by himself but probably less and less fit to be spoken out, less likely to stand the criticism that comes to outspoken words and to stimulate desire for communication. Finally we see the culmination in a sleepless night, and the tantrum at breakfast which felt like a discharge of tension, forced, 'like a feeling took hold' of him, an action by something not quite himself, and in it a characteristic rise to a *contrast* and *compensation* where concrete life brought nothing but failures; and finally a complete avoidance of the trial method which might have balanced his ruminations. Then comes further evidence of queer interferences of thought; some of his mind, his intellect, his power was taken from him—a very frequent interpretation of defect of integration, of blocking and of interferences felt in these states of insufficiency and incongruity of thought and action. Finally there is the development of a most protracted and biologically elementary shrinking into himself with the classical catatonic stupor in which the patient is a mere bundle of tense inactive self-defense, and, after the relaxation of the tension, an emersion of only the most inferior residuals of his interests and stock of endowment and training—a scattered, dilapidated condition rather than

an elementary dementia; inadequacy of emotions and interests and corresponding superficiality and reduction of volition and activity.

I pass over a similar case in which I originally (1905) formulated the evolution of the psychoses in terms of habit-conflicts which shows even more clearly the curve of transformation: masturbation from 9; decline of efficiency at school from 11, at the same time desire to become a teacher, mainly on account of the neatness of dress; frequent headache; two attempts at outside employment, then retired and rather secluded existence at home, excessive shame over the development of a small patch of gray hair; headaches and sleepiness in the morning; later ideas of having a tape worm; an operation for hemorrhoids at 21, with fear that her rectum would close up; then amenorrhœa with worry over her sexual misdeeds; futile attempts to make a better start; she bought paper and pens to become a book-keeper, but never got further; she would insist that she was a 'good girl,' she began to sleep poorly, became afraid at night, and at a party she complained that everybody looked at her. Then came a peculiar religious zeal; she poured kerosene oil on the steps for holy water; she ran to church at midnight; at Bellevue hospital she said—'I hear angels telling me how to pray when I lose my thought'—evidently a happy interpretation of the blocking and feeling of automatism; she scalded her right arm to save the world, soiled herself ('I had to be once more a baby'), developed a peculiar religious-symbolic talk and attitude, at times with senseless moralizing and ecclesiastic utterances with an undercurrent of salvation achieved or longed for, for herself and the world, and passed into a semi-ecstatic catatonic stupor which relaxed after a few years but left the patient in an apathetic so-called 'terminal' state, in the absence of any adequate balancing material before the attack. Again habit conflicts, gradual decline of efficiency balanced by empty ambitions, a series of incomplete attempts to rise, seclusion, hypochondriasis, finally a religious-fantastic compensation of catatonic form and lack of reintegration.

Another type in which the element of tension did not enter in the same manner is that of a young woman stenographer, committed at the age of 21, the child of a thriftless father, and sister of a young man who passed through a transitory mental upset at 20, and two relapses since; the patient had spent several years at a protectory, was bright in her studies, took up dressmaking and shortly after stenography and typewriting. She became, however, indifferent and lazy, remained in bed as late as possible in the morning. She became discontented at home, induced her mother to move into apartments beyond their means and asked her for money to take dancing lessons to go on the stage. October, 1906, she was dismissed from her position because of tardiness in reaching the office. She did not care, tried other positions but could not hold them. When in April, 1907, her brother was sent to a hospital, she became very seclusive, did not leave the house except for business or church. She said later that at that time she learned that her brother masturbated. She read more and more theatrical journals and romantic stories, bought peroxide to bleach her hair, and a preparation to remove the hair from her arms. During the summer of 1907 she spoke occasionally of a young Jew in the office paying attention to her. One night she awakened from a dream screaming; she told her mother that the day before, this young man had put his arms around her neck. She got impure thoughts, confessed having long indulged in masturbation; she admitted that she had thought of this young man in her dream. She gave up her position, feared the men would see her shame in her eyes. Her mother then noticed that her mouth and nose twitched in a peculiar way,

she appeared depressed, threatened to end her life; she was taken to a general hospital where she remained four days and was then transferred to Bellevue and finally to our service.

On admission the patient was quiet, spoke in a subdued voice; at times her expression was rather anxious but she could be easily induced to smile; there was much twitching of the upper lip with dilatation of the nostrils, and quivering of the brow muscles; she did not seem to be afraid yet she uttered many apprehensive ideas and expressed much fantastic horror over the surroundings, asked if they would choke her and bind her down or put her in a cage and make her walk on all-fours. She talked quite freely and often complained of her mind wandering; of inability to concentrate, used many but half-understood words, and remarked that she was not talking connectedly. She referred the talk of other patients to herself and spoke many times of hearing voices. When the nurse rattled her keys, she remarked: "I hear chains—I know I was blind and my brain is turned at the present moment." When asked what her difficulty was she said: "The thing that is uppermost in my mind is a fear that my mother and my brother will go insane if I die—if I—if I am fumigated—this isn't a very connected speech is it?" When asked to explain what she meant by fumigated she remarked: "It means burned up, don't it and thrown to the four winds—you are writing down all I say." When she heard some patients talking in the sitting hall she remarked: "Isn't that terrible those voices out there—that woman is saying I am insane—after this she will punish me—do something terrible to me—put me in a cage and I will have no shame left."

She told of the young man in the office who had put his arms around her and said that later she had a dream in which this young man tried to 'hurt' her; she resisted; after that she had the idea that the young man knew what she had dreamt about. Before she had the dream she had seen this young man make lewd motions and he had winked at her. She thought she had ruined herself by masturbation—"I was ashamed to look at any man—I thought they saw what I had done in my eyes."

The patient was clear as to her whereabouts and her memory was good: she did calculations correctly and promptly.

Her general physical condition was good; she was menstruating when admitted.

Following her admission she was languid, expressed many apprehensive ideas, but did not appear to be in any fear; she complained a great deal of hearing voices, but it was difficult to get her to specify what she heard. She continued to complain that her mind wandered, things were mixed up, she could not concentrate and admitted that bad thoughts kept coming into her mind. Once she asked the physician: "Can't you throw something on me like water to wake me up—I don't know if I am dreaming or not—I used to be tidy but now I am horrible, I don't take care of my person any more." Another time she asked: "Am I crazy, will I ever get over this?" When questioned she was inclined to cover her face, she wished to avoid the physician and expressed feelings of shame.

She was able to do the thinking tests without any special difficulty. grasped the point of what she read fairly readily, but occasionally she gave rather peculiar answers when questioned about her school knowledge and her thoughts at times seemed to be quite scattered.

While in her general demeanor she appeared dejected she never spoke of being sad and she frequently smiled even when uttering anxious ideas—*e. g.*, she remarked with a smile: "I am afraid of being killed." She complained a great deal of feeling strangely, she talked

of shocks of electricity going through her body and of little strings running through her skin; very often she referred to such ideas as 'imagination.' She spoke of talk from inside of her body, voices making lewd suggestions in regard to Christ; she was afraid to repeat what she heard fearing that she would go to hell. She also heard voices from the outside, they told her she would drop dead; she thought it must be wireless telegraphy. She said in regard to the voices—"my brain seems to turn with them, I have no will power."

After a month in the hospital she had become extremely languid and apathetic; notwithstanding continued efforts to gain her interest and to crowd out the ruminative lapses she could not be induced to occupy herself in any way and was very careless about her appearance. Frequently she would throw herself on the floor, whimper and talk of dying. She explained this as a reaction to voices which suggested that she should drop dead. Once she referred to her behavior in Bellevue: "I was very violent and I thought I was Jesus—I jumped on the radiator and sang, thought I was a bird and a parrot." Once she suddenly sprang up from her chair and said that people downstairs were pulling strings; another time she was seen standing by the table running her fingers along the edges; when asked to explain this she said: "*It's all braining.*"

During November, 1907, the patient appeared a little brighter, but she was still extremely languid, listless and apathetic; she expressed many strange ideas and peculiar feelings—*e. g.*, "It seems that people get possession of me in some way through my ears." When asked what she was occupied with she replied: "Something twisted my brain—these people that walk around talk with their feet—my brains have floated down my back" (smiling). When asked if her thoughts were connected she replied: "No, they fly around as though they go out of my ears and float."

Once during the interview she suddenly began to moan, breathed deeply and put her head down on the table. When asked to explain this behavior she said that she felt something in her cheek like 'meat' and thought she heard a young man's name mentioned, a butcher whom she had previously been interested in.

At a later occasion she gave a long statement difficult to understand except as a mixture of reminiscence and sexual symbolism. At present she is largely absorbed by her scattered ruminations, with excessive and forced play of her features. She does not utter her dream-life above a whisper, and in fragments; is unapproachable, and only rises to the level of communication in asking for her hat to go home to her mother, giving the correct address.

Here hereditary deficiency, deterioration of concrete interests with ill-founded and ill-directed aims, and a total missing of her level, with early masturbation and sexual ruminations and poorly controlled ambitions, led away from concrete productivity and from the checks and props of helpful environment and finally to a break of compensation in a sexual experience and a dream, and ideas that men could see her shame, then a pseudo-compensation by fantastic and partly dissociated or hallucinatory (disowned) ruminations in the form of voices and thoughts of uncontrollable and lewd contents, at the same time with queer feelings about her brain, growing preponderance of stilted words, facial expression with exaggerated grimaces and twitchings, discrepancy of mood and thought (laughing when saying that she was afraid of being killed), and a gradual crowding out of the normal thinking and grasp on facts and tendencies by the wholly demoralized gushes of fancy and mental self-abuse. No interests to which to appeal;—progressive deterioration.

While *these* disorders are mainly examples of a gradual and diffuse *deterioration of concrete interests*, the following case presents a striking evolution of a side-tracked complex of specific *longings* to the point of a compensatory fulfillment. A Jewess free of heredity, of rather perverse and stubborn disposition and with outbursts of temper as a child, became an efficient dressmaker, married at 23 a rather inferior man, and was an excellent and efficient wife but excessively jealous. She was crazy for children, but remained sterile. Treatment in 1892 and again 1902 availed nothing; 1902, when run down from a septic infection of the hand, she was told she could never have any children and was greatly upset. She was then subject to vivid *dreams* in which she was attacked. A few months later, a physician suggested an operation for uterine tumor, but she made a scene, called him a bandit and a murderer. Soon after that she began to disclose that she was sure the physician who treated her in 1892 had removed a child from her as she 'was then pregnant.' She said she had seen her boy in the park, asked newsboys to find her Benny, and finally upbraided a physician for *keeping* her child for experimental purposes and was arrested. At the hospital she repeated these fancies in a perfectly orderly fashion, gradually began to promise to let the matter drop and in two months she was taken home. Three months later she resumed her accusations, claimed she was being operated on every night, that doctors damaged her brain away; the papers pictured her as the mother of imagination. She was returned, thought she was pregnant, had nocturnal hallucinatory experiences, spoke of conspiracy, and said the nurses went with her husband. Operative removal of the uterine fibroid had no effect. Gradually the number of her imaginary children grew to ten and more. The husband was declared to have become a millionaire through the money the doctors had given him for the opportunity of experimentation. She again learned to smooth over her ideas, but after a few months at home she returned as daughter of the Queen of Russia, with elaborate systematizations. She is a good worker in the sewing room, discusses her situation pleasantly and coherently if tactfully treated, still asserts that she is pregnant—a typical paranoid compensatory wish-fulfillment, gradually attained after a period of dream experiences uncorrected in her waking life.

From these grave cases many transitions lead over to those in which we see mainly episodic tantrums break through, usually in the form of conflicts, or of wish fulfillments with varying elaboration within the situation, and a number of more or less characteristic traits that mark the disorders as reactions to complexes, faulty and perverted attempts to meet more or less real difficulties and breaks of integration. The chief point is that concrete difficulties and states of tension can be demonstrated and that the course shows a distinct relation between balancing material and further evolution.

We should have to refer further to those cases in which as a rule definite strings of developments appear in a person either originally with difficulties of make-up, or transformed or made unresistive through progressively deteriorating habits of adjustment.

Another case is that of a bright young woman who went through nine months of a catatonic attack with a delirious episode, negativism,

refusal of food, retention of saliva, stereotyped attitudes, echolalia, grimacing, etc.; improved at the end of nine months, went home almost well, but in a few months became sleepless, harped bitterly about fantastic ill-treatment at the first hospital to which she had been taken, again refused food, complained of pain in the left shoulder, improved slowly at the hospital, was again better and might have gone home if the circumstances had permitted; then got worse again and says now, she has no touch with her real environment; her mind dwells on the old story of ill-treatment and she hears remarks on it; she is made to suffer here for another woman; she had refused work for months out of a feeling of aversion and disappointment when transferred to a ward for more chronic patients, but even here was again found at work and much more affable after a review of her situation. Yet this woman is, through fate and the development of her make-up, relegated to the ranks of disappointments of treatment.

Good informants saw nothing peculiar in her and called her efficient, practical, not dreamy. On closer inquiry she was described as very scrupulous about the feelings of others, and equally sensitive to slights, proud of her appearance and reputation, so prudish that she would never undress in the presence of her sister and unusually sensitive about references to sexual matters. She had times when she wanted to be alone and felt nervous and complained of weakness and stomach troubles. Her father had died insane; and a brother (one of five) had had epileptic insanity. The patient herself complains that she *never* was practical; she was a great reader and her sister tells us of a huge scrap book of poetry. From 1905-07 she stayed away from confession. A young woman against whom her sister had warned her, introduced a man to her concerning whom she first spoke with aversion, but who evidently fascinated her under a decided conflict. She was much mortified because she found out that he was a divorced protestant and he left her after having hurt her shoulder by lifting her up playfully, and after borrowing some money. This was followed by another blunder and conflict; although deeply averse to divorces, the patient helped a young woman get evidence for a divorce from a supposed bigamist, worked all her spare time and found out in the end that there had not been any marriage at all. This was followed by her moving to another city; there the conflict preyed on her and a depression came on which rapidly led to a catatonic climax, without the slightest attempt at rapport on the part of the patient or physician, until she was transferred to another hospital and finally discharged after an illness of nine months. The pain in the shoulder which had recurred when she came to us disappeared after an electrical examination of the really existing slight atrophy. The details of the development and the reminiscences from the first hospital were then gone into, but evidently not traced completely to a balance or to the fundamental sore points. The circumstances of her family and the crowding of the hospital necessitated compromises to which she reacted unfavorably several times after a certain level was reached; she relapsed and got out of touch with her real environment. She says distinctly—'my mind is always away from here.' Yet the adjustments in changing the mere ruminations into open discussion and giving space for direct and concrete interests has an unmistakable influence on the patient.

As a purely transitory disorder but with a host of 'pseudo-spontaneous experiences' and feelings of being *made* to do things, I should like to refer to a girl of 23 who made as it were a spontaneous recovery under mere quieting treatment and simple straightening out of the puzzle. Rather inefficient, unable to adjust her work and interests

smoothly, never holding her places as servant long, often sulky, shut-in, and with few friends, without any heredity and denying sexual abuse, the patient had become more self-absorbed about New Year, 1904; in March she had a short attack of articular rheumatism. After it she was even more self-absorbed, was sent on a vacation, after that began to offset the actual condition by claiming that her work was better, that the other girls made trouble and were jealous; at other times she remarked she had something terrible on her mind. An ultimatum about her inefficiency in November finally made her pass into a state in which she was odd, untidy, adding 'thank you' to everything she said, even to her sister. When a man came into the house she ran away, got on her knees and prayed. The next morning, after a few moments of normal manner, she had to be taken to the hospital and disclosed a whole complex of imaginative material, that a young man used to come to her room at night, that she felt she was pregnant, and then stopped feeling the movements; she must have conceived from a spirit, etc. She was run down, admitted dreaming much. Examination spoke against all possibility of her claims. But she described how the feeling developed, how her head felt queer, how she felt her head move, and her hands as if practicing the piano, but without *hearing* orders or voices or music. Even her work was at times forced on her in this pseudo-spontaneous way. One time she lost her speech for a few minutes although she could work her tongue. She gained rapidly, gradually became less apathetic and described the experience with full appreciation and employed herself. She was discharged restored in three months. It is not difficult to see in the whole experience a wish-conflict, traced to a man who lived in the house in the spring but who hardly ever spoke to her. The constitutional make-up with conflicts in her ordinary reactions, a certain amount of ill-health, frequent dreams and finally a tantrum, without the systematic amnesia of hysteria, gradually resolved again without complete ventilation of the entire mechanism. The observation dates from 1905. No catamnestic facts obtainable.

These sketches must suffice. They should show that they deal with developments far from being inconceivable as chains of faulty mental adjustment and far from demanding artificial explanations by specially invented poisons, and a clamoring for invented "things back of it all," if at least we acknowledge the long time and mass of doings and their kind.

These conditions have been grouped together by Kraepelin under the term *Dementia Præcox*, embracing derangements which very often tend to end in peculiar defect conditions, ranging from the not infrequent cases of simple disappointment of parental hopes by apparently promising individuals who fail to make their mark, to cases with rather characteristic mental upsets and characteristic usually progressive apathetic dementia.

These cases do indeed make a group worth distinguishing as a nosological entity and they offer certain common and characteristic traits always carrying a warning that the tendency is towards deterioration. The unfortunate feature of Kraepelin's view is that this possibility or great probability is made to appear as a great dogmatic certainty, dictated by

merely suggestive but not causally correlated signs which are seen in the end-stages and appear also in the very beginnings, as mere empirical ear-marks, merely classified as disorders of emotion and volition, hypothetically due to toxines and brain-lesions, but not reduced to any chain of cause and effect.

The picture as a *whole* makes the *diagnosis*. There are no decisively pathognomonic facts. The deterioration gives the disorder its name; but it need not always be realized; the essence of the process is a hypothetical toxic influence or disorder of metabolism—entirely hypothetical—with definite brain-lesions—also vague and not explained. What he deems essential comes out most clearly in his differential diagnoses, where we find the enumeration: emotional apathy, and specific disorders of will: negativism (mutism, refusal of food, etc.), automatism (catalepsy, echopraxia and echolalia) and mannerisms (grimaces, oddity, stereotypy, verbigeration), silliness, unaccountable and odd acts, etc.

This description, to a great extent taken over from Hecker and Kahlbaum, figured originally under the 'degenerative' disorders. There it would be a mere dispute about words to debate whether or not the early symptoms were evidence of the disease or not. The fact that these early signs *need* not lead to more trouble might still be compatible with calling them degenerative, and the mental factors *might* be admitted to play a more or less active rôle in the development of those cases which progressed further. But in 1896, Kraepelin, taking general paralysis as the paradigm of psychiatry—each disease having a definite etiology, definite course and outcome—included a much wider range of cases in the original group, viz., practically all cases of the simple psychoses which tend not to recover or are apt to deteriorate in the end; and he explained them all as disorders of auto-intoxication with a special assumed brain-disorder. Then the question might have arisen: If we deal with a toxic state where does it become established and when; and what would we have to modify to prevent it? What rôle do the biological reactions play which represent the early symptoms?

Kraepelin purposely declines any idea that special antecedents in the life of the patient are worth considering as causal or even as aggravating dynamic factors. About 20% would, according to him, show some early premonitory signs like seclusiveness, oddity, excessive religious devotion, moral instability, but trusting his *deus ex machina* he sees in this mere evidence of a very early setting-in of the so-called 'disease itself.' As a matter of fact the cases in which early symptoms are found are much more numerous than 20%; as I claimed in 1903, a very *large* number of these cases show what Hoch has

lately called a shut-in personality, specially exposed to inner friction,—a percentage of actual demonstration about as great as that of actual demonstration of evidence or suspicion of syphilis in general paresis. Kraepelin, however, underrated these facts and by absorbing many doubtful and poorly analyzed cases in his group came to suggest that this disease might befall any one, and that it was an autonomous brain-disease.

Even in the hands of the originators of this new large entity embracing all the cases passing into apathetic deterioration and many others that at least tended to deteriorate, the definition of the term is evidently much more fluctuating than the uncompromising theory would suggest. In Heidelberg it has fluctuated from 8% to 52%, and now back to 18% of all the admissions. In the Munich clinic the optimistic tendency is still more on the increase and fewer cases are dubbed dementia præcox on the female side than on the male side. The inevitable conclusion is that between dementia præcox and manic-depressive insanity and simple psychopathy there is an uncertain territory which refuses a categorical arrangement in the easy and simple dogmatic terms that 'some disorders *must* be a deteriorative brain-disease because they early present certain signs also seen in actually accomplished deteriorations' and the claim that it would be futile to make an effort to analyze the data as a whole in terms of cause and effect.

To this empirical and formal conception I have opposed for a number of years a conception which aimed to be less dogmatic and more likely to be conducive to the determination of the facts actually present in the cases in terms of an experiment of nature, in terms of determinable initial constellations, reactions with probabilities rather than fixed laws of termination; in terms of dynamic and possibly modifiable factors and in terms of natural non-dogmatic developments, to quite an extent measurable in advance by the facts at hand in the case and not merely by the intermediary of a dogmatic fate-like noumenon or largely hypothetical construction.

As dynamic factors in these developments there stand out certain activities and states of disturbed balance and regulations which have far-reaching effects upon the mental adjustments themselves, and *incidentally* upon the organic understructure of the personality.

We have so far failed to find any tangible poisons and infections as in any way essential in the process. The extent to which regulative substances akin to hormones may play a rôle and figure as non-mental short-cuts of reaction is a problem for the future to decide. Berkley's claim of hyperthyroidism is not very convincing to one familiar with a goitre-district and large numbers of thyroid affections; Kraepelin's suggestion

that the poison may have some relation to the sexual functions merely flirts with the truth and is so vague as to demand consideration only if actual facts can be adduced and other facts should fail.

On the other hand, we find in evidence factors which are apt to shape or undo a life—specific defects or disorders of balance, with special tendencies and *habitual* ways of bungling and substitutions and a special make-up which is liable to breakdown in specific manners.

In my first formulation of the situation in Toronto (*British Medical Journ.*, Sept., 1906), I started from the paradigm of *complete action* as the function which gets more and more disorganized by first trivial and harmless *subterfuges* or *substitutions* which, in some individuals, lead further, become harmful and uncontrollable, tend to assume types of definite anomalous mechanisms, unintelligible and crazy if viewed apart, but more or less intelligible as a string of actions substituting, and often missing, an efficient adjustment to concrete and actual difficulties.

These substitutions constitute the symptomatology and chains of events which we have found in the cases described and which I need not rehearse and analyze before you owing to the shortness of the time and because the facts in the cases described are more trustworthy than verbal formulas. Suffice it to say that we meet neurasthenia, hysteria or psychasthenia-like substitutions, or mere dilapidations of interests or states of conflict or depressions of a morose, *topical* character—usually with one or more initial tantrums of the character described,—and delusional developments either with episodes of ruminations and giggling and the like, which may absorb more and more of the patient's actual life, or catatonic developments, or paranoia-like delusional states, all with a number of ear-marks: in the main freedom of the hysterical haziness and tendency to systematic amnesia, but evidence of conflicts of reaction, of blocking, of peculiar automatic interferences, *i. e.*, evident disorders of the highest integrations, and fantastic ruts especially in the sexual or religious spheres and their symbolic elaborations, with very frequent dissociation of the personality and pseudo-spontaneous experiences; further, a growing divorce from the concrete environment, a deterioration of interests and perversion of impulses and actions.

I must pass over the systematic attempt to account for the various symptomatic mental and non-mental developments, as I am convinced that the complete account of the cases with their concrete settings and developments is bound to be the best and only safe basis for deductions, although I realize that the brevity of the sketches left untouched many legitimate

queries, especially those about the non-mental or so-called physical components of the reactions, and the possibility of accounting for them with habitual substitutions and habit-conflicts. I only wish to refer to the catatonic reactions which are especially often mentioned, not only as being oftenest connected with certain brain lesions, but as being unexplainable from the psychological side. The catatonic reaction is by no means so far from yielding to a psycho-biological interpretation. It is a breaking down of normal conduct and adaptation too closely related to what is seen in hypnotic states and in mystic fancies; too directly like stages in religious symbolism and feelings of submission to influences by mystic powers to be compared with what happens in organic psychoses. In general paralysis and arteriosclerosis and senile deterioration, it is not the *synthesis* into a personal integration that is *most* lacking, but the *material* used in the synthesis is decreasing, through lack of memory, judgment and the range of capacity, without any distinct following of the lines of functional cleavage in the process of disorganization. In dementia præcox the dissociations follow the lines of functional and topical complexes. The very frequency with which especially catatonic reactions appear outside of the actual deteriorations, though preferably in dementia præcox, would corroborate their interpretation as a specific functional reaction type possibly founded on a phylogenetically very old reaction partly of protection or partly of mystic surrender. If they are apt to appear occasionally in organic psychoses, the same holds for manic-depressive and other more essentially psychogenetic reactions. It is, however, certainly significant that catatonic disorders are *most* apt to accompany the *traumatic* forms of organic disorders, such as also produce hysterias and other after-effects most likely connected with a functional shock. In the simple *dilapidation* and the paranoic developments, the psychological staging is too much in keeping with the situation and the harmonious evolution on prevailing premises, to create serious doubt against an essentially functional interpretation of the evolution and, also, of the lesions which may be found.

Sizing up the disorder in terms of a break in the working of conflicts, of balance rather than in terms of an autonomous disease of the brain, will stand and fall with the extent to which the initial data allow us to predict the course of nature's experiment, a point concerning which only the publication of casuistic material will give sufficient proof. Our work with these principles warrants the conclusion that while general paralysis is *relatively* incalculable in the *details* of its course, and certainly remarkably *independent* of mental determinants, the fluctuations observable in dementia præcox are decidedly too of-

ten accounted for by renewed up-sets and tangles and irritation of idiosyncracies, and that the prognosis of the ultimate tendency is remarkably often foretold, so that of the cases interpreted as actual deteriorations but few surprise us with a recovery, and those that *do* recover are as a rule specified at the outset as cases merely akin to this group worth naming by the end-stage, but with varying amounts of balancing material. Such a disorder is, to be sure, as little open to *absolute* prediction as life's vicissitudes, and a continued test of estimates of events in the light of ultimate results gives one a certain reserve and modesty; but, with it all, the conviction grows that the factors depended on in the estimate of the make-up and in the ratio of the reaction and balancing material, are really *factors at work*, and leave less and less space to a craving for what is 'back of it,' instead of attention to what is the 'go.'

Where a break or morbid reaction has once set in, it is very difficult to bring relief directly. The fundamental shutting in and the whole mechanism enables the preoccupations to live themselves out and to exclude interference. Automatic resistance against the most natural impulses frustrate even the occasional pathetic spontaneous appeals of the patient for help. The best procedure is to tide over the acute tangle with as much tact and ease as possible, to promote relaxation, and to relieve the situation wherever that can be done, bearing in mind the facts obtained referring to the upsetting factors, the probable complex-constellations and prevailing physical disorders. As soon as the patients feel that they meet with help instead of an argumentative and corrective attitude they can be led considerably when the time comes or where the difficulty has not led to complete blocking. Then a positive re-education in the form of habit-training and of readjustment has to set in. It is obvious that experience brings a certain divination and that individual capacity plays a decided rôle in the straightening out of the difficulties, both during the tangles, and in ultimately marshalling the forces to a more practical unity and level again; it is also obvious that we cannot be very optimistic in most cases, as little as when we try to win over our less unbalanced neighbors to a better mode of thought, belief and conduct and behavior.

We owe to our European guests, Professor Freud and Dr. Jung, the demonstration that what is at work in the centre of the stage is a complex or group of complexes consisting of insufficiently balanced experiences in various ways modified by symbolism. Their ingenious interpretations have made possible a remarkable clearing up of many otherwise perplexing products of morbid fancy, in ways the discussion of which, no doubt, I had better leave to their lectures.

Yet, if I interpret their accounts correctly the reason why only few persons create these complexes and fewer yet develop them to a disastrous form and often to a deterioration, is mainly left to heredity or finally to toxines, whereas I would prefer to adhere to my attempt to define the responsible factors as far as possible in terms of prophylactic suggestiveness, in terms of untimely evocation of instincts and longings (acting as fatally as premature destruction of naïveté), and ensuing *habit-conflicts* with their effects on the balance of the person, and on the sum total of mental metabolism and actual doings and on the capacity for regulations in emergencies. In some cases the *habit-disorders* preponderate in the side-tracking and the curbing of leading interests and creation of disastrous substitutions; in others, definite complexes play a special rôle and as a rule the sizing up of the disposition must consider both factors. In practically all cases the scope and funds of mental deviation form a consistent evolution and offer the safest material for prognosis and practical handling.

For all I can see the main obstacle to a wider acceptance of a functional theory in terms of habit and complex conflicts and definite responses thereto, is on the one hand the habitual or intentional lack of the necessary penetration into the life of the patient and family, and on the other hand, the readiness of the physician to turn to set interpretations and to reiterate authoritative statements with a certain pedagogical self-sufficiency. I refer especially to the traditional rut shown by physicians when they have to meet the question of habit-disorders, such as masturbation, which invariably leads to reasoning in a circle by calling the disorder a symptom of a disease and evading the possible rôle in additional abnormal developments instead of to a frank inquiry into the facts and difficulties in the case. Further, there is perhaps also a more or less legitimate aversion to any extreme dogma, using too exclusively the sexual origin or the weight of complexes, and special displacement mechanisms, and an aversion to certain other 'atomistic' types of psychopathology, and especially also the fact that so many spontaneous recoveries occur and also many failures under almost any procedure.

The most serious cause for relapses into opposition to psychogenetic interpretations is the blind acceptance of any anatomical findings as definite evidence of an autonomous disease, after the paradigm of general paralysis. And to this point I wish to give a brief discussion.

The lesions found by Sioli and others are very different from those of general paralysis in their nature *and* as to autonomy of origin. They are most akin to fatty involution of the brain tissues, probably as incidental to the disorder of function as is

the brown atrophy of the heart, the fatty degeneration of muscles or of the liver. The one disease in which disorders similar to those in dementia præcox, and even more marked, have been seen, is Huntington's chorea which is a striking instance of familial insufficiency of the nervous system, and hardly a product of a toxic disorder. The occasional late recoveries of apparently demented patients and the peculiar clearing up of some cases during intercurrent diseases—in which the most *vital* instincts of self-preservation and of complex-free family interest are brought out again—would certainly make one doubtful about the “profound deteriorations of the cortex” being on an autonomous basis as in general paralysis. Until we know much more about the amoeboid neuroglia and the protagon degeneration seen in dementia præcox and in Huntington's chorea and probably elsewhere, we certainly do well to leave open the question whether a disorder of anabolism and catabolism incidental to the prolonged and often profoundly vitiated attitudes and defects of balance is not sufficient to explain the findings (which are possibly as incidental to special chronic disorders of function as the finding that Dr. Hodge has established in acute fatigue states), or to what extent they are perhaps short-circuits; that they are incidental to a broad frame, seems unshakable and the more we teach the physician to think in terms of what is demonstrable in the case, the better for him and for the patient and for prophylaxis and for the formulation of further problems of investigation.

The lesion in general paralysis is of a totally different kind, depending on a previous infection with syphilis and forming a peculiar infiltration of the brain vessels, similar to what happens in the African sleeping sickness, but accompanied by additional degenerative processes in the brain tissue. This exogenous disturbance leads to death within a limited number of years, and accounts for certain fundamental symptoms of dementia of a kind quite different from that in dementia præcox. In addition to that, there are, however, symptoms not common to all cases, such as the development of exaltations or depressions or delusional states, sometimes following certain traits of dementia præcox. These superadded psychotic symptoms have been attributed to different localizations or distribution of the characteristic lesions. A careful inquiry into this question on the material in the literature and our own observations of focal general paralysis shows, however, that the focal lesions may give aphasic attacks or neurological disturbances, and occasionally precipitate epileptiform reactions with amnesic phases, fugues, and states of bewilderment; but the psychotic symptom-complexes occur without any regularity. In one case of Alzheimer a dementia præcox-like disorder of

paranoid hallucinatory developments was connected with special affection of the left parieto-temporal region, but the patient had had an earlier attack six years before the suspicion of general paralysis arose. Such a case as this and a number of others suggest strongly that these *usually* psychogenetic disturbances depend more definitely on the previous mental make-up, even in the general paralytic, very much as has lately been admitted by Bonhoeffer and Homburger in certain alcoholic and exhaustive disorders—Homburger being a pupil of the Heidelberg school but under the influence of the master of functional pathology, Krehl.

In view of these considerations it is unintelligible that analogy with general paralysis could be strongly enough founded, to excuse a recent writer on the insane in Massachusetts who urges or sanctions on this ground a plea of medical ignorance with the following remarkable conclusion: "Until we have learned more by continuous study of the causation and pathology of dementia præcox, curative measures will be most fruitfully employed in the manic-depressive and toxic cases, to increase the percentage of recoveries and diminish the number of deaths."

The comfort of working under the cover of fatalistic and analyzed conceptions of heredity, degeneracy and mysterious brain-diseases—and the relief from responsibility concerning a real understanding of the conditions at hand, and concerning the avoidance of preventable developments—is a powerful and unconsciously cherished *protection*, very rudely disturbed by these conceptions which make the physician partly responsible for the plain and manageable facts. I deny that fatalism is inevitable, without admitting that my conception should imply unwarranted optimism. It is merely a return to the facts at hand which will prepare us all the better for the actual work, and pave the way towards prophylaxis where something can be done. The position is, however, equally important in the utilization in psychological teaching. There probably is a certain comfort in arranging the courses within a narrow range of laboratory problems. Unfortunately, that does not always train the student's sense in using the foot-rule of ordinary life with any degree of accuracy or conscience, when he passes to more complex domains. Scientific accuracy in one field does not guarantee a critical attitude in the fields of nature's experiments which are complex and cover larger spans. If we make the student wade through a mass of rather artificial psychological laboratory work, and on the other hand, equally artificial philosophical puzzles, we would leave him in the end without help and training to meet some essentials in life. Even a non-technical knowledge of the facts in some mental

patients is bound to widen the horizon and would to my mind be an intrinsic part of any course or programme of psychology (as good as, or better than, an abstract course on mind and body). Without its concrete lessons many events appear like puzzles and are unduly treated as such. This stands out glaringly in a recent book on psychotherapy which makes the reader divide the attitude of appreciation and the attitude of physical explanation without helping him to unite them again; which contrasts the subjective and objective and the purposive and the causal view without bringing them to the common denominator of experience again; which urges him to split psychiatry and psychotherapy—and therein fails to be helpful in the very task of sane instruction, namely, that of integrating disconnected facts into sane 'organized common-sense.' Familiarity with the concrete events in nature's experiments would reduce the longing for these artifacts.

I have on purpose avoided entering upon the details of many excellent modern trends of psychological investigation in our field. I wanted to make a plea for the broader *frame* of things. This frame must be grasped with an understanding of the broader elements in the disorders with which we deal. Within this frame the details get their perspectives. In the theory-ridden physician and in the ultra exact psychological laboratory worker, I should like to awaken the natural instinct of curiosity concerning the keenly interesting broader biological settings brought out by the mental disorders and destinies discussed. I should like to make all feel the sanctity and paramount interest of the concrete cases. I cannot resist recalling what is so well expressed in the recent Presidential address of the great physicist, J. J. Thomson, in his appeal to the mathematician to avail himself of the power of the concrete. He says: "Most of us need to tackle some definite difficulty before our minds develop whatever power they may possess;" and we cannot deny that the field of habit conflicts and of far-reaching and complex emotions and longings gets its most wonderful representatives in disease. Ribot opens his last study with the remark: "Le meilleur procédé d'expérimentation en psychologie, à mon avis, est la maladie avec ses désordres." Diseases are the most crucial experiments in man. Here the momentous things occur in a way which might well supplement the man-made experiments of our laboratories and suggest problems in a way which really go at the causal relationships vital to the student, vital to any layman who wants to know what psychology is and does, and vital to the physician who wants to help also where help would rarely come without him, and may even be too late with him, as long as we fail to make sure of prophylaxis.

We are, I believe, justified in directing our attention to the factors which we *see at work* in the life-history of the cases of so-called dementia præcox. We are justified in emphasizing the process of a crowding out of normal reactions, of a substitution of inferior reactions, some of which determine a cleavage along distinctly psychobiological lines incompatible with reintegration. Psychobiological analysis and reconstruction furnish us the essential material, and progress is to be expected from a frank and unprejudiced weighing and use of this material including its non-mental components rather than from the stereotyped lesion-pathology and the dogmatic nosological principles when they become intolerant.

I could not have had a more delightful opportunity to present a discussion of the essential facts in favor of a dynamic conception of dementia præcox than this occasion as I realize that my development has to no small extent been influenced by the spirit at Clark University, its genetic attitude and the liberality in admitting the facts for investigation whether they seemed to fit preconceived plans or not, and its strong faith in the selective capacity of interest and in an unprejudiced inquiry with or without laboratory methods, but always with an interest in the conditions under which reactions develop.

THE PAST DECADE IN EXPERIMENTAL PSYCHOLOGY¹

By E. B. TITCHENER

I am to speak in this hour of the course and progress of experimental psychology during the past ten years. The psychological laboratory has, as you know, had but a short history; and the modern psychologist counts in decades, as the historian of human thought counts in centuries. It is, I hope, not out of place to remind you that even the century is an artificial unit; when we think of the philosophy of the eighteenth century we certainly include Locke in our list of writers, although Locke died in 1704; when we think of the psychology of the nineteenth century we certainly do not include Fechner, although Fechner came after Herbart. The century, none the less, serves a useful purpose, because a hundred years, three human generations, are as a rule sufficient for the testing of an idea, for its establishment or its final rejection. Of our own unit, the decade, we can say no more than that, in the precocious development of the later-born sciences, it is at least a period long enough to warrant an inventory or stock-taking, from which we may gauge the trend of interest in the immediate past, formulate problems for the present, and possibly infer the direction of inquiry in the immediate future. The ten years whose psychological activity I am to review have, however, a special claim upon the historian. Beginning in the nineteenth, they lead us at once across the line of secular division into the twentieth century; they embrace the culmination of the one and the first effort of the other; and I cannot resist the belief that all the sciences, old and young alike, approached this twentieth century with a certain self-conscious expectancy which, however difficult to appreciate to-day, will presently be seen to have exercised a marked influence upon the intellectual movement of the time. It is, then, if I read the signs aright, a period of unusual interest and of especial scientific significance that has formed the second decade in the life-history of Clark University.

Nevertheless, as I approach the topic of this lecture, what is

¹ Lecture delivered at the celebration of the twentieth anniversary of the opening of Clark University, September, 1909.

uppermost in my mind is a sense of irreparable loss. When the cable brought the bare news, last February, that Ebbinghaus was dead, just a month after the celebration of his fifty-ninth birthday, the feeling that took precedence even of personal sorrow was the wonder what experimental psychology would do without him. You are all familiar with Ebbinghaus' work; to some of you, as to myself, his death has meant the loss of a friend: those who had not known him, but had looked forward to his promised address from this platform, have missed an experience that should have remained a lifelong memory. I shall not here attempt an eulogy: that is unnecessary. But I must remind you that Ebbinghaus' qualities were precisely those that, in its present stage of evolution, experimental psychology seems most to need. What characterized him was, first, an instinctive grasp of the scientific aspect of a problem,—scientific as distinct from philosophical, in all the protean meanings of that latter term; secondly, a perfect clarity of thought and of language, the expression of thought; and thirdly, an easy mastery of the facts. I say mastery; but the truth requires a stronger word. There was about Ebbinghaus a sort of masterfulness; he never did violence to the facts, but he marshalled them; he made them stand and deliver; he took from them, as of right, all that they contained; and with the tribute thus exacted he built up his theories and his system. This, I say, was the example that we needed, in a time when psychology still appears helplessly entangled with theory of knowledge; when the standard of scientific writing, so far as literary style is concerned, is deplorably low; when theory is impatient of fact, and the facts themselves are scattered and inco-ordinate. I believe, indeed, that Ebbinghaus' *Grundzüge*, already in its incomplete form a centre of widespread influence, was destined to a place of leadership; I have sometimes thought that, with allowance made for changed conditions, it might prove as important to experimental psychology even as Wundt's *Physiologische Psychologie* or Brentano's *Psychologie vom empirischen Standpunkte*. But Ebbinghaus is gone; and with his passing our science has sustained the most grievous loss that it has been called upon to bear since Delboeuf undertook that fatal journey to Munich thirteen years ago.

These brief remarks, inadequate as you must feel them to be, will at any rate in their spirit and intention command your assent. I pass now to another preliminary matter, upon which agreement is hardly to be expected. For agreement implies, in the first instance, a common point of view; and my own standpoint, which is that of pure science, or the desire for knowledge without regard to utility, is in all likelihood shared

only by a small minority of this audience. Moreover, agreement within the domain of pure science presupposes a certain measure of progress, a platform of assured results; and to that point, perhaps, experimental psychology has not yet attained. Nevertheless, while I anticipate that you will reject my conclusion, I trust that you will also remember the general attitude and point of view from which it is derived.

If, then, one were asked to sum up, in a sentence, the trend of psychology during the past ten years, one's reply would be: Psychology has leaned, very definitely, towards application. And if the questioner were thereupon to look for proof of this statement, he would find it confirmed not only by the range and variety of current practical work, but also and more particularly by the incursion into the field of practice of men whose training and previous interests might naturally have held them aloof. I shall not try to indicate the surprises that this movement towards application had in store for the theoretical psychologist; still less shall I try to set before you with any fullness the evidence for the strength and universality of the movement itself; I mention only a few typical facts. In 1903 Meumann opened the pages of his *Archiv. f. d. ges. Psychologie* to studies in applied psychology; and in 1905 Wundt alleges the preponderance of these studies as a principal reason for the foundation of the *Psychol. Studien*. In the same year 1903, Stern began the publication of his *Beiträge z. Psychol. d. Aussage*, which by 1907 had developed into the *Zeits. f. angewandte Psychol. und psychol. Sammelforschung*. Meinong writes in 1904 that experimental psychology, now brought into touch with the needs of practical life, is on the way to become a popular science; and expresses the hope that the contact of theory and practice may grow constantly closer. In 1906 Jung published the first volume of his *Diagnostische Assoziationsstudien*. In 1907 Meumann gave us two volumes of lectures on experimental pedagogy, with the promise of a system to follow. In 1908 Binet formally devotes his *Année psychologique* to the cause of practice; psychology is to be laid out and aligned with reference to practical and social questions. The present year has seen the publication, in English, of two popular works—Münsterberg's *Psychotherapy* and Watt's *Economy and Training of Memory*—which by diversity of aim and content as well as by form and style bear witness to the scope of application. I have mentioned such things as occurred to me: the more omissions you remark, the more securely will my thesis be established.

Now we have all heard it said—and said in connection with this practical tendency of recent work—that experimental psychology cannot hold its men; that its problems are not large enough to satisfy our intellectual demands; that the ex-

perimental psychologist will inevitably turn, sooner or later, to æsthetics or theory of knowledge, to physiology or general biology, to education or therapeutics. There is, in fact, some leaven of truth in that statement, and there is much untruth. Remember that no experimental science can hold its men beyond a certain term of life; remember that a large proportion of our students come late into the laboratory, and that they bring no habit of experimentation with them; and remember also the natural indolence of mankind, and how much easier it is to write an essay than to plan and carry through a series of experiments. But notice, on the other hand, that the experimental psychologist oftentimes has no choice allowed him; he may even hold his office under a foreign title; he is called to represent, in the economy of the university, a group of more or less closely related disciplines; and while no man to-day would claim for himself the title of physicist, it is not thought amiss that a philosopher or an educator should make a by-work of experimental psychology. All these things must be taken into account,—though I doubt if any one of them goes to the heart of the matter. The essential point is, surely, this: that many men, who are not by temperament psychologists, pass through the psychological laboratory on the way to their proper goal. Some are attracted by curiosity, by the mere charm of novelty; some desire in more serious mood to see and to understand. For all, psychology lies, convenient and accessible, at the cross-roads of human knowledge; and we who dwell there can only be grateful to those who for a season share our labors, while we hope, as soon as may be, to bid the casual visitor godspeed.

Under these conditions, the diversion into practical channels of energy which would otherwise have been expended in the service of the laboratory must be regarded as a definite loss to pure science; and it is from the standpoint of pure science that I am now speaking. You will reply, perhaps, that there are compensating advantages. For those who apply psychology testify, in the act, to the soundness and relative maturity of psychological ideas. And if we may judge by the experience of the older sciences, of physics and physiology, the trial of these ideas in practice will react upon the ideas themselves; application will discover new problems, which must be referred back for solution to the experimentalist. I recognize the advantages; but I do not think that they offset the loss. Every one, for instance, who has followed the history of science, knows that successful application is but a very imperfect measure of the validity of a theory; material improvement may go astonishingly far under the guidance of some scientific hypothesis which later generations

roundly pronounce erroneous, and which shows, indeed, but a glimmer of the later found truth. Pragmatism, as it was at first interpreted by its critics, and as it seems at first to have been intended by some, at least, of its propounders, could never pass muster with men of science, who see how well ideas may work upon how precarious a basis of fact. But, if we leave generalities aside, and consider the actual status of experimental psychology, we are still forced to the conclusion that this first argument overshoots the mark; it ranks as final achievement what is, for the most part, no more than tentative suggestion. And as for that other argument, of give and take, action and reaction between theory and practice, I confess that I am a little tired of it. Some day, if analogy may be trusted, it will hold; to-day it is but the expression of a pious hope. For application, if I read the documents fairly, has proceeded in two principal ways: it has borrowed and transformed some approved psychological method, or it has adopted and popularized some assured psychological result. I have nothing but respect for the men who by ingenuity and hard work make so much out of so little; but I cannot believe that either of those procedures will, by reaction, bring any considerable gain to science.

These, however, are merely counter-arguments, rebuttals; and it behooves a minority to be aggressive. Let me add, then, a positive statement. So far is experimental psychology from any general readiness to furnish ideas for application, that applied psychology has been obliged to think out ideas for itself; and so far is applied psychology from reliance upon the parent discipline, that some of its most widely used and most strongly emphasized ideas contravene established scientific principles. The notion of a quasi-mechanical dissociation, for example, or various modern forms of the doctrine of the subconscious,—these ideas are both foreign to the spirit and inadequate to the status of experimental psychology; they are stumbling-blocks in the path of scientific enquiry, obstacles that it will take time and labor to overcome. Not only, therefore, has the movement of the last few years, by its withdrawal of men and its substitution of practical for theoretical interests, brought with it a loss to science; but it has also, in the manner indicated, placed positive hindrances in the way of scientific advance.

That is the situation, as it appears from my point of view: others will see it differently. It is, in any case, a situation that must be accepted; and we have now to consider how, because or in spite of it, experimental psychology, the theoretical psychology of the laboratory, has fared during the past ten years.

I wish that I could proceed systematically. But, as we have already seen, psychology is not ripe for systematization. Let that be said emphatically:—and when you hear criticism of the claims advanced or the promises made by experimental psychology, remember, in all justice, that it has been said emphatically, and be at the pains to discover whether the claims and the promises come from psychology itself or from some less responsible enthusiasm. We must, in fact, proceed topically, though we shall naturally begin with the three fundamental topics of sensation, affection and attention.

In the sphere (1) of *sensation*, the most striking fact to record is the revival and extension of Fechnerian psychophysics. We have but to exceed our time-limit by one year, and we may start out with Martin and Müller's work upon the differential sensitivity (1899). Then follow Foucault's *Psychophysique* in 1901, Wundt's fifth edition in 1902, Lipps' *Grundriss* in 1903, Müller's *Gesichtspunkte und Tatsachen* in 1904, Aliotta's *La misura in psicologia sperimentale* and my own *Experimental Psychology* ii. in 1905, Lehmann's *Psychologische Methodik* in 1906, Wundt's sixth edition and Urban's *Statistical Methods* in 1908—to say nothing of a number of authors (Ament, Fröbes, Mosch, Holt, Laub and others) who have published single papers of considerable theoretical interest. Truly, there is hope for a generation which, in the midst of practical activities and despite the clamor for results, can find the time and the men to pursue in this persistent way the study of scientific method! And the outcome? The outcome is that, on the level of ordinary quantitative work, we now know enough to check our methods; we can prove our sums, as the children say; our figures have a definite scientific meaning and a general scientific setting. There will still be differences of opinion; there will still, no doubt, be controversies; force of habit would see to that, for some years to come, even if there were no questions of fact that remained unsettled. But it is a great deal, in science, to be sure of your ground, to have sifted out personal opinion and speculative guesswork from observed fact and genuine problem; and I think we may say, without undue optimism, that psychophysics has now reached that stage of self-knowledge, and has thus been lifted, with no fear of relapse, to a higher plane.

This, as I said, is the most obvious advance to record in the recent psychology of sensation. You could, indeed, hardly expect that progress should be as clearly marked on the side of quality, where the mass of established fact was already very large, and where—thanks to the co-operation of the physiologists—productive work has been continuous. If, however, I were called upon to mention a single noteworthy

event, I should select the re-issue of Hering's *Lehre vom Lichtsinn*, in which the veteran author has set himself; after the lapse of a generation, to theorize afresh the whole realm of visual sensation. Another gift from physiology is the third volume of Nagel's *Handbuch*, which will henceforth be to our students what Hermann's *Handbuch* has been to ourselves. And if we feel a natural regret that the old order should change and the authorities of our youth lapse into forgetfulness, let us at the same time recognize that the change does but emphasize the debt of psychology to its sister science: a debt, moreover, that must remain unpaid so long as physiology refuses the sole return that we can make, and physiologists decline instruction in psychology. For the rest, I may roughly indicate the progress of the decade by reminding you that we have to-day no satisfactory theory of sight or hearing, of taste or smell, of pressure or temperature; the generalizations that worked so well for science a few years ago, and that still do good service in the text-books, are in the minds of those who know breaking down under the stress of newly discovered facts; and the passing of established views is only one aspect of a process which, looked at from the positive side, leads to reconstruction. But again, if I must be concrete, I can choose a single instance: the revival during the last few years—in the hands of Meumann, Becher, Murray—of experimental interest in the organic sensations. When we remember the importance of organic sensation in the affective life, its importance as the vehicle of sensory judgments in psychophysical work, the part it plays in the mechanism of memory and recognition, in the motives to action, in the primary perception of self, in many of the complex formations that go by the general name of thought: when we remember the systematic questions that hinge upon it,—the question of the elementariness of pleasantness-unpleasantness, of the relative range of sensation and image, of what is called affective memory, and so on; we can hardly fail to see that these modest beginnings promise to fill up a great and painful gap in our psychological knowledge. It is too early to ask for results. It is something if we realize, in a fairly definite way, the difficulties of method that the experimenter must overcome before results are obtained; or rather,—it is everything that the experimental study of organic sensation has actually been attempted.

I undertook to speak of progress, not of problems. Yet I do not like to miss this opportunity of public reference to a problem which, although it has already been approached here and there, in a general way, seems to me just now to call pressingly for detailed treatment: I mean, the problem of sensible dura-

tion. In our analytical study of complex formations,—perceptions, perceptual groups, and especially total consciousnesses,—we must, I am convinced, make greater use of the temporal attributes of sensations. Is there any one who has not, time and again, been puzzled and surprised that experiences which felt so differently should come out so similarly in analytical terms? Well, experimental psychology, in its natural concern for intensity and quality, has unduly neglected the other aspects of sensation, and among them this aspect of duration. Wundt has set us a good example; he has employed duration, as an instrument of analysis, in his recent discussions of feeling and emotion; and I venture to suggest that the example is one to be followed in all departments of laboratory work.

This mention of Wundt takes us easily to our second introductory topic, the topic (2) of *affection*. Wundt, as you will remember, published in 1896 the first draught of his tridimensional theory, and in 1900 his *Bemerkungen zur Theorie der Gefühle*; Stumpf published in 1906 his paper *Ueber Gefühlsempfindungen*. These dates are significant: for they imply that the experimental psychology of feeling, which begins with Féré in 1887 and Lehmann in 1892, attains its full development in the decennium now under review. The threshold year 1899 is important here, by reason of Lehmann and Angell and Thompson, as we saw it to be important for psychophysics. Then follow, in quick succession, the experimental studies of Zoneff and Meumann, Brahn, Titchener, Gent, Bonsor, MacDougall, Orth, Boggs, Gordon, Störing, Kelchner, Hayes, Urban, Johnston, Keith, Shepard, Alechsieff, Salow, Kaestner, Nakashima,—studies that differ in method and in result as they differ also in scientific value, but that are all alike aimed directly upon an experimental control of the affective reaction. How near we have come to a stable psychology of feeling, I confess that I do not know; the smoke of controversy lingers, and it is impossible to get a clear view of the field of battle. Perhaps, for a little while, we shall none of us know; perhaps, we must await some new strategic movement to reveal the strength or the weakness of the various positions. I read, only the other day, that the attempt to build up feelings out of sensations has now been completely abandoned,—and I remembered, with some amusement, how many psychologists are still at work upon that task. No! in all such statements the wish is father to the thought, and the thought is but the wish become dogmatic. A more cautious estimate might, I think, venture upon three propositions, but could hardly go beyond them: first, that we have transcended, for good and all, that pseudo-Darwinism which, running directly counter to the intention and the accomplishment of Darwin's great work,

offers a facile teleology in place of scientific explanation, and deems the affective problem solved when it has written 'useful' for 'pleasant' and 'harmful' for 'unpleasant'; secondly, that our working hypotheses are adequate, intensively and extensively, in range and in detail, to serve as guides to experiment; and, thirdly, that the investigations so far published, inconclusive as you may judge them to be, nevertheless prove that experiment is possible. And this, you must remember, is the sole source of anxiety in an experimental discipline,—whether, when a question has been asked, the method of experiment can furnish a valid answer. So there is always, at the outset, a certain rush and hurry of research, and a certain immaturity in publication; but, the main point settled, and experiment shown to be feasible, the problem is put upon the regular waiting list, to be taken up at leisure. That is, as I see things, the present status of affective psychology. In the meantime, Wundt has made his tridimensional theory the basis of a psychology of language; and Stumpf has made his theory of a centrally excited accessory sensation the basis of a genetic psychology of the tonal and musical feelings: and while the two theories are incompatible, both of them appear to work.

I pass (3) to the psychology of *attention*, which I take to be in yet another characteristic stage of development, less advanced than the psychology of sensation, but more advanced than that of affection. Historically, such a state of affairs is but natural: for experimental work upon attention was begun by Wundt in the sixties, while the experimental study of affection dates only from the late eighties of the last century. And the bare fact that it thus occurs to one to measure progress by lapse of time is significant and encouraging, since time means nothing for science until methods have been thought out and men are trained to apply them. Nevertheless, I must not give you the idea that our knowledge of attention has increased continuously, little bit by bit, as the years have passed; that there is any historical analogy of that kind between sensation and attention. On the contrary, the doctrine of attention has shown very markedly the characteristics that certain psychologists ascribe to attention itself: it has fluctuated, risen and fallen again; wave of interest has alternated with period of indifference. Ebbinghaus wrote, in 1902, that attention is a real perplexity in psychology: "die Aufmerksamkeit ist eine rechte Verlegenheit der Psychologie;" and I think that he himself felt what he wrote; his treatment of attention is a little perfunctory, as if the composing of those sections had been a disagreeable duty. The same sentence stands in 1905; and, indeed, the whole discussion has remained practically unchanged

from the first to the second edition of the *Grundzüge*. Yet I believe that, at this latter date, one of our attention waves was gathering to a head. At any rate, the next three years saw the publication of as many systematic treatises: Pillsbury's book appeared, in French and English, in 1906 and 1908, and in 1907 came Dürr's *Die Lehre von der Aufmerksamkeit* and Roerich's *L'attention spontanée et volontaire*. It is true that the French and German authors are largely concerned with practical issues; but they deal with attention from the side of theory also; and Pillsbury's work, the most comprehensive of the three, devotes only a few pages to educational matters. Now, these men must have found something to say: and while their teaching is, in good part, inferential, even speculative, it rests, none the less, upon a broad basis of actual observation. There are two things, in particular, that to my mind attest the progress of the past decade. The phenomena that we have been wont to refer to as the fluctuations of attention are now shown, pretty definitely, to be peripherally conditioned; I, at least, am unable to put any other interpretation upon the facts; and the psychology of attention thus loses a chapter which has always been a fruitful source of confused thinking. For there are two main uses, in current investigation, of the term attention. In the practical studies of economy of learning, and the like, attention has an energetic meaning; we hear of energy of attention, distribution of attention, quickening of attention, recovery of attention, and so forth. All these phrases are readily intelligible, since they simply give precision to the usage of everyday life; and they are also sufficient for the writers' purpose, since they designate, unequivocally, certain well-marked events or phases of the process of learning. At the same time, it must be remembered that they are employed, casually and by the way, in the course of inquiries which are not themselves directed upon an analysis of attention. The point is important, if it is a little obvious, and I pause a moment to illustrate it. In work upon the affective processes, we often refer a judgment roundly to 'association;' in work upon association itself, we often record an 'attitude' of perplexity or doubt or hesitation as a link in the chain of interconnected processes: the abstract terms 'association' and 'attitude' meet the conditions of the inquiry, and no one can quarrel with their use. But no one, either, can doubt that they presuppose a psychology; they imply a foregone psychological analysis: and what holds of them holds similarly of the term 'attention.' Now, when we turn to the second, analytical meaning of attention, we find that our concern is with the clearness or vividness of conscious contents; energetics drops out of sight. But you will see that the notion of a fluctuation of attention, of apperceptive waves, suggests the ebb and flow of

energy, so that we are constantly tempted, even when engaged upon a purely analytical task, to take attention for granted, to employ it as *datum* in the very context which we have arranged for it as problem. It is in this way that confusion has arisen, and it is on this account that we may rejoice to refer to sensation phenomena that, ever since the days of N. Lange, have been attributed to the attention.

I said that there were two things that bore witness to the progress of the decade; and the second, which I can but briefly mention, is the revival, with good promise of success, of the attempt to measure the attention, to give quantitative expression to distinguishable degrees of clearness. It is still too early to discuss this work in any summary way. The impulse to it comes, most appropriately, from the Leipsic laboratory, and Wirth's further researches will not only be awaited with eager interest, but will also, as we may hope, arouse activity in other quarters.

So much, then, for sensation, affection and attention. Our topical survey will bring us next, I imagine, to that mixed medley of formations which is included under the general term (4) *perception*. It is altogether impossible to review, in the time allotted to me, the work accomplished in this field of vast range and uncertain limits: I have tried, as I was in honor bound to try, and I have given up. You must be content with a purely subjective and arbitrary selection. I found, as a matter of fact, after refreshing my memory of some hundred experimental papers, that I had been most impressed by the work of Benussi on optical illusions, on the perception of time, on the comparison of spatial distances and temporal intervals; it seems to me that Benussi's treatment of the apprehension of form, his notion of the inadequacy of idea, his doctrine of accidental figure, and so on,—that all this is of positive value in itself, and will bring forth fruit in the future; and my judgment has remained the same, as study followed study, although I am by no means able to accept Benussi's interpretations. The Austrian school—however widely we may differ from them on systematic questions—have, indeed, made notable contributions to this chapter of psychology. For the rest, these ten years appear to have hardened and emphasized the special characteristics of American work upon the perceptual problem. Perception, in the cis-Atlantic laboratories, has been given a biological setting and perspective, and the theories of perception have been motor theories. I do not think that any good has come from the intermixture of biology; on the contrary, there has been a tendency to substitute final for efficient causes, and general considerations for exact psychophysical determination.

I am also heretical enough to think that our current motor theories are both premature and one-sided: premature because they far outrun our knowledge of the motor mechanism, and one-sided because they directly attach the motor to the receptive organs, and forget the disjunctive office of the cortex. But I do not, on this account, underestimate the achievement of Dodge and Münsterberg and Judd.

I pass from perception to the cognate subjects of (5) *recognition, memory and association*. Ebbinghaus, as I need not remind you, published his experimental study of memory in 1885; and there was no lack of experimental papers in the nineties. But consider our initial year, 1900. That year saw the issue of Müller and Pilzecker's *Exp. Beitr. z. Lehre vom Gedächtniss*; of Laura Steffens' work *Ueber die motorische Einstellung*, and Lottie Steffens' *Exp. Beitr. zur Lehre vom oekonomischen Lernen*; of Kemsies' and Netschajeff's articles on the memory of children; of Smith's *Rhythmus und Arbeit*. Our decennium began well; and it ends as it began; for it was only the other day that we received the second and concluding portion of Wreschner's elaborate memoir *Die Reproduktion und Assoziation von Vorstellungen*. In truth, this investigation of memory and association illustrates and accords with the tendency of the time. Ebbinghaus himself conceived his problem rather in a practical or psychophysical than in a psychological spirit; and, in the light of what I have already said, you will find it natural that his example should be followed,—all the more natural, indeed, since in this particular field psychophysics and practice are closely and obviously related. There are, however, in reality, three distinct problems. We may aim at a psychology of memory and recognition and association; that is, we may seek to record our experience, to trace introspectively the arrangement and course of consciousness as we remember or recognize or associate. We may aim, secondly, at a psychophysics; we may trace and measure the action of the reproductive and perseverative tendencies, evaluate the parts played by the reproductive and the recognitive factors, work out formulas of the same kind as that first formula of Ebbinghaus which represents retention as a function of time elapsed. Or we may aim, thirdly, at an applied psychology; we may lay down rules for the training and the economical use of memory. There can, now, be no doubt that the two latter aims have, in the period which I am reviewing, taken precedence of the first. But psychology has by no means been neglected: Cordes' analytical study of association appeared in 1901; Whipple's analysis of the memory image and the process of judgment in discrimination, in 1901-2; Gamble and Calkins' experiments

upon recognition and comparison in 1903; while Kuhlmann, who worked for the most part in the Clark University laboratory, has published in 1906, 1907 and 1909 a series of articles devoted especially to the introspective characterization of the memory consciousness. If to the results of these enquiries we add the indirect results of the psychophysical experiments—and indirect or secondary results have always been of great importance in experimental psychology—we have a considerable body of knowledge, even though it is not as yet either organized or complete.

I come next (6) to *action*. The reaction experiment we have had always with us, ever since the psychological laboratory came into being; it is an experiment that has been sorely abused, in word and in deed; yet so many are its possibilities, that interest in it has never flagged; and we are now beginning to see that it furnishes an invaluable instrument of psychological analysis. The decade opens with a number of technical papers: W. G. Smith in 1900 and 1903, and Judd and his collaborators in the Yale laboratory in 1905, have sought to determine the nature of the reaction movement; and Alechsieff in 1900, and Bergemann in 1905, have essayed new methods of quantitative evaluation. But this, the technical or psychophysical aspect of the experiment, has been overshadowed by the psychological. In 1905, Ach published a book entitled *Ueber die Willenstätigkeit und das Denken*,—a book which reports and discusses the employment of what the author terms, a little redundantly, the 'method of systematic experimental introspection' in a series of experiments upon simple and compound reactions. He could, indeed, hardly have chosen a more promising field. For although Kuelpe said as long ago as 1893 that "reactions are nothing else than exact types of . . . voluntary action, . . . so that their mere duration is but a small part of their psychological significance," and although Wundt has repeatedly endorsed this statement, no one before Ach had made any serious attempt to build up a psychology of volition and action upon the introspective data which the reaction experiment affords. In brief outline, Ach distinguishes a fore, mid and after period of reaction: the fore period extends from signal to stimulus, the mid or principal period from stimulus to reactive movement; the after period is a time of indefinite duration, but certainly lasting several minutes, which follows immediately upon the conclusion of the experiment. The method of systematic experimental introspection then requires that the events of the fore and mid periods be introspectively examined, as a whole, during the persistence of the perseverative tendencies in the after period. I shall have to refer, in

my second lecture,¹ to the results of Ach's investigations; it is enough now to say that those results have thrown a flood of light upon the action consciousness, and that they are happily confirmed by the outcome of other experiments, especially by the work of Watt, undertaken independently and for a different purpose. Hitherto, the chapter in our text-books devoted to the psychology of action has been a medley of evolutionary biology, physiology, and vague psychological generalization; henceforth, it will contain facts of psychological observation.

I wish that I could say as much (7) for *imagination*. But indeed, whether we begin with the elementary process, the image, or whether we go to the other extreme, and regard imagination as the general name for a group of typical formations, as a concept co-ordinate with memory, we must surely say that experimental psychology is to-day hardly over the threshold of the subject. My own belief is that we must start out with the image, and determine, for instance, whether image quality is co-extensive with sensation quality, and whether imaginal difference is adequate to sensible discrimination. More especially must we make a serious effort to resolve the paradox of the image: that it is so readily confusable with sensation, and yet so easily and certainly distinguished. Much may be hoped, I think, from the current studies of imagery, and from the incidental results of work upon thought and voluntary action. Thus we have recently found, in my own laboratory, that on one important point the popular psychology of memory and imagination inverts the facts: the image is available for memory, not because or in so far as it is stable and permanent, but precisely because it is instable and breaks down; because it admits of fusion, telescoping, substitution, and thus enables us to carry a great deal of experience in condensed and abbreviated form; while the imaginative mind, at any rate on the average level, is not a mind whose images change kaleidoscopically, producing by their instability new and still newer mental combinations, but is, on the contrary, a mind equipped with an almost photographically persistent imagery, which may be contemplated and arranged at leisure. This is natural enough, when you have discovered it; but it is hardly what you would have argued *a priori*. I might cite a number of similar results: but the facts stand in isolation; in the main, imagination is virgin territory, and awaits as it invites the pioneer.

Of the more complex (8) *affective formations* we can say but little until we have an assured psychology of feeling. I

¹ "The Psychology of Thought." This Lecture is not here reprinted, as the author has recently published a more extended discussion of the subject.

remarked, earlier in this lecture, that Wundt has made significant use, in his theory of emotion, of the temporal aspect of mental processes; and I think that the future experimenter will do well to take the hint. But we cannot analyze emotion and sentiment, with any prospect of final success, until we are agreed upon the nature and number of the affective qualities.

It is a relief to turn from these topics, of imagination and emotion, to the problem (9) of *thought*. We have found various things that may with some truth be called 'characteristic' of the experimental psychology of the last ten years: the revival and extension of psychophysics, the focalizing of the affective problem, the emergence of a tangible psychology of attention, the establishment of the laws of memory. Nevertheless, if I were asked to name a single line of investigation that, more than any other, has characterized the decennium, I should not hesitate to select the experimental studies of the thought-processes, most of which we owe to the Würzburg laboratory. Not that these researches have been confined to Germany: on the contrary, Binet in France, Woodworth in the United States, Bovet in Switzerland, as well as Marbe and his successors at Würzburg, have all attacked the same problem; though it is true that the German work has been the most thorough and the most persistent. Here is a new departure in experimental psychology; concept and judgment and inference, the last refuge of the rational psychologist, have been ranged alongside of sensation and association, introspectively analyzed and made subject to the chronoscope. I shall endeavor, in my next Lecture, to give you some idea of methods and results, and to point out the most promising paths of future enquiry.

So I finish the principal part of my review. If I have omitted anything of consequence, or if I have seemed to do injustice to any department of work, I must ask for pardon and correction; I have spoken with the utmost possible brevity. It remains, now, to say something of the extensions of the experimental method beyond the limits within which the present discussion has moved. What of individual psychology; of the psychology of the minor abnormalities—sleep, dreaming, hypnosis; of experimental æsthetics? What, last but not least, of comparative psychology?

I have time only for a word or two. Individual psychology, which was first systematized by Stern in 1900, is, in its modern form, one of the chief witnesses to the value of experiment. It furnishes the key to many, otherwise inexplicable differences of result, and it promises to allay many of the standing controversies of the text-books; there can be no

doubt that it will play a part of steadily increasing importance in the immediate future. The psychology of the abnormal seems, on the contrary, if I may venture an opinion, to have failed somewhat of its theoretical promise; recent interest has turned toward application. Vogt's method of 'direct psychological experiment in the state of hypnosis' has not helped us, as some had hoped it would, to an affective analysis; and although Martin and Ach have brought hypnosis into the laboratory, it does not appear probable that their example will be generally followed. Experimental æsthetics has shared in the recent horizontal expansion of psychology at large. A review of the work done upon colors and spatial forms, which appeared in the sixth volume of the *Année psychologique* (1900), mentions no more than five investigators; and there is an interval of more than twenty years between the first and the second, between Fechner and Witmer. But when Kuelpe came to prepare a similar review for the 1906 Congress of Experimental Psychology, he found a wide extension of subject-matter, a whole armory of methods, and a very considerable body of results. No doubt, the time is still distant, if indeed it is fated to arrive at all, when experimental æsthetics shall bear to a *System der Aesthetik* the relation that experimental now sustains to systematic psychology. In the meanwhile, it is satisfactory to know that, in this as in other directions, the American laboratories are doing their full share.

Again, however, there is something still more characteristic to follow: I mean, of course, the rapid growth of an experimental psychology of the lower organisms, a growth that is evidenced by the issue of books, the manifold publication of technical studies, and the invasion of our laboratories by various unwonted forms of animal life. The present status of comparative psychology has already been set forth by an authority far more competent than myself; and I desire now simply to give expression to my personal feelings, as the director of a laboratory primarily intended for the investigation of human consciousness. Personally, then, I welcome the animals, both for my own sake and for theirs. For my own, because I think that the comparative work exerts a wholesome influence upon the humanist. It is difficult to lay one's finger upon any definite point of indebtedness; but I am sure that many of my ideas have been quickened and clarified by acquaintance with the problems and methods of comparative research. And for the sake of the animals: because a comparative psychology can be built up only by men who have had their training in human psychology. Of that fact there can be no manner of doubt. The zoölogist may, and probably does, excel the psychologist in his mastery of biologi-

cal technique. But psychology also has its technique: psychology has its attitude, its point of view, its permissible and its impermissible questions, its methodological equipment, a technique that it takes time to acquire; and if you lack it, you may observe animal behavior with all the patience and all the accuracy conceivable, but you will not attain to a comparative psychology. Until such time, therefore, as experimental psychology forms a recognized part of every man's general biological training, training in physiology and zoölogy and the sciences of life at large,—until such time, the place for the animals who are to reveal the range and character of their mental endowment is the psychological laboratory.

I am almost at an end; and I have given you, I am afraid, little more than a string of names and a few general remarks; though, after all, one cannot do much in an hour. There is, now, one further point that I desire to discuss before I close. I have taken it for granted, throughout this lecture, that the primary aim of psychology is the analysis of mind. Yet a great deal has been written, of late years, against psychological analysis. Consciousness, we are told in effect, is a living continuum; but the analyst kills in order to make his dissection; and, after killing, he is unable to restore the life that he has taken, to show consciousness in its original integrity. This argument, if it were taken seriously, would apply to biology as well as to psychology, and would banish the muscle-nerve preparation and the microtome from the biological laboratory. But, indeed, it rests only upon misunderstanding,—a misunderstanding due in part to temperamental reaction, in part to the pressure of history and tradition. When the physiologist describes a tissue as 'composed' of muscle fibres or nerve cells, nobody takes him to mean that the fibres and cells existed first, in isolation, and that they were presently brought together, by some law of organic growth, to constitute the tissue. What grew was the tissue itself, which the physiologist now finds, in his *post mortem* examination, to consist of the cells or the fibres. Nevertheless, the analytical psychologist is supposed to generate his mind by allowing sensations to fuse and ideas to colligate, precisely as the physiologist might be supposed to generate a muscle by allowing the fibres to 'constitute.' In reality, to charge the analytical psychologist with deriving mind from the interconnection of the mental elements—and how often and how recklessly has not that charge been made!—is sheerly to misunderstand the purpose of analysis in the hands of those who use it.

The scientific legitimacy of the analytical attitude is, then, beyond dispute. Whether the results of analysis, in the sphere

of mind, are of 'value' is another question, and a question whose answer depends upon what one is disposed to consider valuable. What is psychology 'for?' If the object of the psychologist is to know mind, to understand mind, then it seems to me—in view of the overwhelming complexity of mind in the concrete—that his only course is to pull mind to pieces, and to scrutinize the fragments as minutely as possible and from all possible points of view. His results, in synthetic reconstruction, give him the same sort of intelligent grip upon mind that the analytical results of the physiologist give him upon the living body. To approach the study of mind without analysis would, in fact, be nothing less than ridiculous; and, in fact, no one does it. The most ardent advocate of mental integrity can follow up only one mental aspect or one mental function at a time.

I conclude that analysis is not only valuable, but indispensable to psychology; and I contend that many of the current arguments urged against psychological 'atomism' betray a woeful misunderstanding of scientific methods, and that much of the current depreciation of analytical results betrays a like misunderstanding of the aim of scientific psychology. But, in saying these things, I am only repeating what has been more aptly said by Ebbinghaus. And as I began this lecture by deploring the loss which Ebbinghaus' untimely death has entailed upon our science, so I cannot end it better than by reference to the admirable sanity which marks his treatment of the wider issues of psychology in the first volume of the *Grundzüge*.

AN EXPERIMENTAL STUDY OF IMAGINATION¹

By CHEVES WEST PERKY

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INTRODUCTION

The word Imagination and its cognate forms are familiar both in everyday speech and in the technical language of psychology. In neither context, however, have they the established position enjoyed by the correlative term Memory. Under these circumstances, it seemed worth while to enquire into the psychological status of Imagination, to attempt an experimental control of certain of the experiences thus denominated, and by these means to work towards a definition and delimitation of the concept. The present study is no more than a first beginning, but we hope that its results are sufficient to justify the recourse to the experimental method.

Alongside of the experimental work, which will be described later, we undertook three preliminary enquiries: a somewhat

¹From the Psychological Laboratory of Cornell University.

casual investigation of the meanings attached to the word Imagination by ordinary educated persons; a canvass of literary usage; and an analysis of the treatment of Imagination in current psychological text-books and monographs.

(1) *Ordinary Usage.* The verb 'to imagine' is used with many shades of meaning. We found, however, two fairly characteristic conversational uses: 'to imagine' is either to set an imaginary something before the mind's eye, or it is, more generally, to make an hypothesis, to suppose something of an unusual or difficult kind. Instances of a simple sort would be: 'Now you are to imagine a very long lane,' and 'Could you imagine anything worse?' The turn of mind which is commonly called imaginative is suggested by various proverbial expressions: counting chickens before they are hatched, crossing bridges before you come to them, building castles in Spain, etc. The imaginative person is also credited with a certain lightness or instability of temperament, though perhaps the only feature common to all mental constitutions classed popularly as imaginative is the negative one of a lack of thorough-going stupidity. Sometimes, as we have indicated for the verb, 'imaginative' has a visual reference; as a rule, however, this reference appears to be no more obvious to the speaker than it is in the case of the substantives 'insight' and 'foresight.'

(2) *Literary Usage.* One of the commonest uses of the word 'imagination,' in literature as in ordinary conversation (where, however, if our notes are to be trusted, it is far less common than the adjective and the verb), is to denote a tendency to distort facts, to embroider, to romance: cf. the phrase 'lively imagination.' Akin to this tendency is the ability to live in a subjectively colored world unlike the gray world of reality, a temperamental optimism or pessimism. With a very slightly different shade of meaning, imagination becomes the ability to see everything in a rosy light; or, again, the faculty of seeing things in the very high color that strong feeling paints. In some cases, the power of sympathy, of feeling with and for, is actually called imagination. Rich endowment on the creative as well as on the affective side of human nature produces the reformer, the prophet, the revolutionist, the visionary. Emphasis is often laid on this creative factor, the power of invention; imagination implies fluidity of associations, quickness of wit, the faculty of forming new combinations, the ability to see likeness (the one in the many) and to draw distinction (to see the many in the one). Finally, we note that the imaginative person is, with approximately equal frequency, described as the player, the dreamer *par excellence*, the purposeless idler, and as the possessor of an unusually high degree of concentration and voluntary control.¹

(3) *Psychological Usage.* It is easy to overestimate the significance of a formal definition. At the same time, the framing of a definition marks a stage in the development of our knowledge of a subject,

¹ This paragraph is a condensed summary of the results gathered from a fairly wide study of poets, essayists, divines, and articles dealing specially with the Imagination. Of the latter we may mention, by way of illustration: H. Maxwell, *Imagination*, Blackwood's Edinburgh Magazine, cl, 1891, 576; R. C. Witt, *The Imaginative Faculty*, Westminster Review, cliv, 1900, 217; What is a Strong Imagination? *The Spectator*, lxii, 1889, 506; *The Imagination and its Development*, *ibid.*, lxxv, 1890, 372; *The Conservation of the Imagination*, *The Saturday Review*, lxxxviii, 1900, 576; *The Imagination*, from Lectures by J. R. Lowell, *The Century Magazine*, xlvii (N. S. xxv), 1893-4, 716; J. Sully, *Studies of Childhood*: i. *The Age of Imagination*, *Popular Science Monthly*, xlv, 1894, 323. The greater part of the authors consulted wrote in English; but sporadic search among French and German writers did not reveal any striking differences in their use of the term.

while the definition itself has at least the value of a classificatory rubric. When James Mill writes that "an imagination is the name of a *train*; . . . nor is there any train of ideas, to which the term imagination may not be applied,"¹ we have our bearings within a self-consistent and comprehensive psychological system. It seems fair to argue, then, that the casualness and vagueness of the definitions of Imagination offered in current text-books and monographs imply a real uncertainty as to its psychological status. Imagination is "the representative function of mind,"² or "the faculty of reproducing copies of originals once felt"³; to imagine is "merely to think of an object,"⁴ or "to construct with images."⁵ More specifically, the creative imagination "consists in the property images have of gathering into new combinations through the effect of a spontaneity"⁶; "it is the power of developing an object by being absorbed in it; it is the power of self-development of an object when we live it or live in it."⁷ Or it may be both of these things at once: imagination "is the art, spontaneous or reflective, of forming syntheses or mental combinations," while it is also "the power of giving or attributing existence to the representations, of transforming them into beliefs or actions, in a word, of objectifying them."⁸ These and similar statements are plainly partial or tentative; they have their useful place in the discussions in which they occur; but they certainly do not possess any general validity. The clearest and most positive definitions that we have found are the following three. "Imagination in the wider sense," says Höfding, "is identical with the power of ideation; . . . in the narrower sense Imagination is the power of forming concrete ideas."⁹ Here is the modern form of the representative faculty. Wundt modifies the definition, to make imagination a "form of apperceptive activity;" imagination is "a thinking in particular sense ideas," a "perceptual form of intellectual elaboration."¹⁰ Finally, Baldwin introduces the affective factor; "imagination is the affective or felt apprehension of relations among images."¹¹

Is there, then, any constitutive *differentia* of the imaginative consciousness, any marked character in which its contents differ from the contents of other typical consciousnesses? Most psychologists are silent upon this point. Külpe notes the "oscillation and migration, the shrinking and expanding, of the subjective images, when they are the result . . . of the free and fortuitous play of imagination;"¹² and Wundt emphasizes the "sensory vividness and picturableness of its ideas."¹³ It would, however, be unsafe to generalize upon either basis.

Other principles of distinction have, indeed, been proposed. Thus

¹ Analysis of the Phenomena of the Human Mind, i, 1869, 239.

² J. M. Baldwin: Handbook of Psychology; Senses and Intellect, 1890, 213.

³ W. James: Principles of Psychology, ii, 1890, 44.

⁴ G. F. Stout: Analytic Psychology, ii, 1909, 260.

⁵ L. Arréat: Mémoire et imagination, 1895, 156.

⁶ T. Ribot: Essay on the Creative Imagination, tr. 1906, 330.

⁷ W. Mitchell: Structure and Growth of the Mind, 1907, 353.

⁸ L. Dugas: L'imagination, 1903, 3, 308.

⁹ H. Höfding: Outlines of Psychology, 178; Psychologie in Umrissen, 1887, 223 f.: "das Vermögen zur Neubildung konkreter Vorstellungen."

¹⁰ W. Wundt: Outlines of Psychology, tr. 1897, 262; tr. 1907, 298; Human and Animal Psychology, tr. 1896, 316; Physiologische Psychologie, iii, 1903, 577, 632; Mythos und Religion, i, 1905, 5 ff., 62 f.

¹¹ *Op. cit.*, 242.

¹² O. Külpe: Outlines of Psychology, tr. 1909, 185; Ueber die Objectivierung und Subjectivierung von Sinneseindrücken, Philosophische Studien, xix, 1902, 508 ff. Münsterberg speaks similarly of the *Unbeständigkeit der Vorstellung*, and of the *bunte Wechsel der Phantasiegestalten*, Grundzüge der Psychologie, i, 1900, 341. Cf. F. Paulhan, Psychologie de l'invention, 1901, 7.

¹³ Physiologische Psychologie, iii, 1903, 632, 634.

Wundt writes that the distinguishing mark of imagination is "the mode of connection of ideas;" "in all cases connection takes place according to a definite plan."¹ We have "an essential mark of the apperceptive process in the positive characteristic . . . that it depends on a voluntary synthesis."² Jodl speaks, in the same sense, of a "voluntarily effected reproduction."³ Külpe enters on more familiar ground in the statement that "imagination is characterized . . . by the realization that the given ideas present something new."⁴ We find many passages to this effect: "the creative imagination requires something *new*—this is its peculiar and essential mark;"⁵ "its two essential characteristics are originality and power;"⁶ "the component ideas . . . are brought into new combinations;"⁷ and so on. Bain thinks that the "grand peculiarity" of imagination, as distinct from thought, is "the presence of an *emotional* element in the combinations;"⁸ a point of view which, as we have seen, is also represented by Baldwin. Ribot declares that "the imagination is subjective, personal, anthropocentric; . . . the understanding . . . is objective, impersonal, receives from outside."⁹ This last is evidently a logical distinction, and tells us nothing of the mental processes involved. And the other peculiarities noted, voluntary synthesis, novelty of combination, and the presence of an emotion, are after all marks of difference within the intellectual processes themselves, and cannot be considered as ultimate. If the presence of images might distinguish imagination from rational thought, as a voluntary synthesis distinguishes it from memory, Wundt would clearly make his point; but it is unquestionable that rational thought may itself proceed in terms of images.

Failing to obtain a satisfactory definition or *differentia* of Imagination, we may turn to the statistical method, and enquire how, in general, it is treated in the standard psychologies; what part-processes are said to make it up; under what general headings, or in what general context, it is placed. It is somewhat surprising that current psychologies rarely give a separate chapter to Imagination; in many of them, it does not even share a chapter heading; occasionally, the term does not appear in the index; not infrequently, its importance in the context is indicated merely by the use of a capital letter.¹⁰ It is also, perhaps, somewhat surprising to find that its discussion may appear, almost as if by chance, under the rubrics Image or Representation, Thought, Feeling, and Will.¹¹

As a type of *intellectual* consciousness, Imagination is usually treated under the general heading of Ideation, or more definitely under Representation, Reproduction, or Centrally Excited Sensation; occasionally under Association or Thought-connection.¹² It is almost always in close proximity to Memory. Emphasis is laid rather on its complexity of contents than on its uniqueness as an intellectual formation; we read, as in any discussion of the intellectual processes at large, of the part played by affective, volitional, and motor or kinæsthetic factors. A few instances must suffice. Ladd and Ribot stress the motor aspect:

¹ *Physiologische Psychologie*, iii, 1903, 631.

² *Outlines of Psychology*, tr. 1897, 261; tr. 1907, 299.

³ *Lehrbuch der Psychologie*, 1896, 509; ii, 1903, 161 f.

⁴ *Outlines of Psychology*, tr. 1909, 188.

⁵ T. Ribot: *Creative Imagination*, tr. 1906, 5, 11.

⁶ L. Dugas: *L'imagination*, 3.

⁷ T. Ziehen: *Introd. to Physiological Psychology*, tr. 1895, 168. Cf. Mitchell, *op. cit.*, 350 f.; A. Bain: *Senses and Intellect*, 1868, 570.

⁸ *Op. cit.*, 599.

⁹ *Op. cit.*, 10.

¹⁰ Nine such cases were actually counted.

¹¹ Actual count from 20 works, chosen at random from our list of authorities, gave the ratio 16:16:15:12.

¹² So T. Lipps: *Grundtatsachen des Seelenlebens*, 1883, ch. xx.

"especially must we insist upon the prominence of motor consciousness in the neural conditions of productive imagination;"¹ "my aim is to extend [the motor] formula, and to show that it explains, in large measure at least, the origin of the creative imagination."² Bain and the French authors insist on the emotional aspect; there is an "emotional condition of originality of mind in any department;"³ we must recognize the existence, "beyond images, of another factor, instinctive or emotional in form."⁴ Baldwin's paragraphs recur constantly to preference, intention, volition, regulation of will;⁵ Ribot finds in voluntary activity the closest analogy to the creative imagination;⁶ Jodl remarks that the conduct of association by the attention passes over, almost indistinguishably, into the activity of imagination.⁷

Imagination, it is generally agreed, must in some way be concerned with images. But opinion differs as to what constitutes an image; some writers appear to include in that category even the vaguest of abstractions. "These ideas of the imagination may be generalized," says Ziehen.⁸ "When used as a sign, the image may be made so schematic as to contain nothing irrelevant; and the abstraction may be so great that the sign loses the character of a sample, and becomes an arbitrary sign like a word."⁹ "The image, deprived little by little of its own characteristics, is nothing more than a shadow."¹⁰ The point is even made that "imagination exercises itself as well in the domain of pure ideas or abstractions as in that of concrete realities."¹¹ It is plainly impossible to generalize Wundt's criterion of sensory vividness. Despite the terminology of imagery, the trend of opinion seems to be not only that "between the creative imagination and rational investigation there is a community of nature,"¹² but also that the line of separation between thought and imagination is shifting and uncertain.¹³

There is a marked difference between the treatment of imagination as an intellectual, and its treatment as an affective or volitional consciousness. Nearly as many writers offer the latter as offer the former treatment; but they do not offer it as a matter of course; they are plainly on the defensive. Hence they urge, with great insistence, that "the *emotional* factor yields in importance to no other;"¹⁴ that "it is a character of imaginative creation . . . almost essential that it be preceded, prepared for and accompanied by affective phenomena;"¹⁵ that "imaginative" are identical with "emotion-ruled combinations;"¹⁶

¹ G. T. Ladd: *Psychology Descriptive and Explanatory*, 1894, 410.

² T. Ribot: *Creative Imagination*, 3, 99, 113, 248, 318 ff.

³ A. Bain: *Senses and Intellect*, 593, 599 ff.; *Mental and Moral Science*, 586; note to James Mill's *Analysis*, i, 245 f.

⁴ Ribot: *op. cit.*, 7 f., 12, 31, ff., 79 ff.; L. Arréat, *Mémoire et imagination*, 122, 167 f.; L. Dugas: *L'imagination*, 3 ff., 7; F. Paulhan: *Psychologie de l'invention*, 28, 43.

⁵ *Senses and Intellect*, 223 ff.

⁶ *Creative Imagination*, 9 ff.

⁷ *Lehrbuch der Psychologie*, 1896, 508; ii, 1903, 161 f., 166. Cf. G. Spiller: *The Mind of Man*, 1902, 496 ff. (imagination as attention-determined); Ribot: *Creative Imagination*, 86 (identity of attention and imaginative synthesis); Dugas: *op. cit.*, 100; Paulhan: *op. cit.*, 67. Stout (*Analytic Psychology*, ii, ch. xi) treats of imagination together with belief.

⁸ *Introd. to Physiological Psychology*, tr. 1895, 168.

⁹ W. Mitchell: *op. cit.*, 364; cf. 371 and 185 f.

¹⁰ Ribot: *op. cit.*, 18.

¹¹ Dugas: *op. cit.*, 1. As a rule, however, the authors halt on the hither side of this statement: cf. Dugas: 3 f. 105, 198, 334; Ribot: *op. cit.*, 18, 184 f., 193, 207 ff., 236, 251 ff. 92 f.; Arréat: *op. cit.*, ch. vi (cf. pp. 11, 103); Ladd: *op. cit.*, 415; Mitchell: passages, cited above; Lipps: *op. cit.*, 469.

¹² Ribot: *op. cit.*, 29.

¹³ Cf. Ladd's discussion: *op. cit.*, 431 f.

¹⁴ Ribot: *op. cit.*, 31.

¹⁵ Paulhan: *op. cit.*, 28.

¹⁶ Bain: *op. cit.*, 602.

that "thought is the representative or cognitive apprehension of relations among notions; imagination is the affective or felt apprehension of relations among images."¹ And emphasis on the *volitional* aspect, if again somewhat less frequent, is at least as insistent. "Memory images change under the influence of our feelings and volition to images of imagination:"² "the active imagination involves the exercise of will in some of its forms, whether it be the positive attempt to control the images of fancy or the merest supervision and direction of their play;"³ not only is "imagination in the intellectual order the equivalent of will in the realm of movements,"⁴ but the two are identical so far as the part (an important part) played by movements is concerned;⁵ "intellectual creation is altogether analogous to voluntary activity;"⁶ "the image is, under an elementary form, a complete volition."⁷ —

It must be understood that the summary here given is inadequate, not only to the subject of the psychology of imagination, but also to our own survey of that subject. We have made no mention, *e. g.*, of such matters as the distinction between passive and active imagination; and we have refrained from multiplying quotations. At the same time, we hope that the impression left upon the reader by the above paragraphs is essentially correct.

Our preliminary study has left us with mainly negative results. We have found the term Imagination used very variously both in ordinary speech and in the psychologies, we have found scarce and inadequate definitions, no distinctive mark or marks of a reliable kind, and a pretty even distribution of emphasis among the four psychological rubrics of Idea, Thought, Feeling and Will. The very character of these rubrics, which are an inheritance from the faculty psychology, shows that no really analytical work has been attempted; psychologists have relied, for an account of imagination, upon their own self-observation and reflection, upon tradition, upon the requirements of a system. This is not to say that their accounts are wrong; on the contrary, they probably contain a good proportion of truth; but it means that there is no way of separating fact from fiction; we must accept an image-theory, or an affective theory, or a motor theory, or a volitional theory, in large measure by faith, or we must try to keep in the safe path by eclecticism and compromise.⁸

These are precisely the circumstances under which, if we may judge by historical analogy, assistance may be expected

¹ Baldwin: *op. cit.*, 242.

² Wundt: *Outlines of Psychology*, tr. 1897, 261; tr. 1907, 299.

³ Baldwin: *op. cit.*, 226.

⁴ Ribot: *op. cit.*, 9.

⁵ *Ibid.*, 11.

⁶ Paulhan: *op. cit.*, 6.

⁷ Dugas: *op. cit.*, 100.

⁸ No one, for instance, can read without psychological profit such a work as E. Lucka's *Die Phantasie, eine psychologische Untersuchung* (1908); but no one, we suppose, will doubt that the writer's analyses and classifications are provisional and temperamental, rather than objective and final.

from the experimental method. But experiment must begin with the relatively simple, and not with the complex. The following sections are offered as a contribution towards that *Elementaranalyse der Phantasie*, which Wundt has prescribed as the foundation of a psychology of Imagination.¹

§I. A COMPARISON OF PERCEPTION WITH THE IMAGE OF IMAGINATION

The object of our first experiments was to build up a perceptual consciousness under conditions which should seem to the observer to be those of the formation of an imaginative consciousness. A visual stimulus was presented, gradually and with increasing definiteness, while the observer was asked to imagine the object whose color and form were thus given in perception. Hence by 'image of imagination' we here mean, not the elementary image-process that is co-ordinate with sensation, but such an image arises in a mind of the visual type at the command, *e. g.*, 'Shut your eyes and think of an orange.'

The Cornell Laboratory possesses a dark room, 5 x 6.5 m., which is set longwise to the middle of a light, gray-tinted room of considerably larger size. The dividing wall contains, at its centre, a window of 1 x 1.5 m., which is filled with a sheet of ground glass, and which may be closed on the light-room side by two swinging shutters that can be made to stand at any angle. For the purposes of the present experiment, a black cardboard screen, with a central opening of 36 x 36 cm., was placed in the dark room immediately behind the ground glass. Facing the glass, in the dark room, was a projection lantern, whose arc-lamp was replaced as occasion required by various types and powers of incandescent lamp. The light room is profusely supplied with incandescent lamps; and after some preliminary trials with daylight, we decided to use this artificial light throughout the experiments.

Our first problem was to determine the color-limen for the various stimuli that we intended to employ, and we succeeded in making this determination for a certain red, orange, deep yellow, light yellow, green and blue. The observer sat, in the light room, at a distance of about 8 m. from the ground-glass window. It was the task of the experimenter to vary the luminosity of the lamp in the lantern, the screens of ground and colored glass, colored and colorless gelatine, and white tissue paper, necessary to reduce the light from the lantern, the distance of the lantern from the window and from these various media, and (on the other side of the ground-

¹Mythus und Religion, i, 1905, 17 ff.

glass window) the illumination of the observation room, in such a way that the open square should appear just noticeably colored, without there being any such glow or shine upon the glass as could suggest the presence of a source of light behind it. After a great deal of empirical testing, this end was attained, with a satisfactoriness and a precision that we ourselves—discouraged by repeated failures—had at last not dared to expect. Serial determinations of the limen of hue were obtained from Professor Titchener and the writer; a number of less systematic observations were made by Professor Bentley. The apparatus, we may repeat, was clumsy and empirical; but it worked (apart from errors of manipulation, to which we later refer) with admirable sureness and delicacy. We were especially on the watch for changes of tint; but we were able to eliminate them, positively for ourselves, and at least so far that they escaped notice for all of our observers.

The limen was thus determined as a diffused flush of color over the open square. The next step was to shape this flush into the representation of some object of perception. We prepared a set of black cloth-covered screens, in which were cut the forms of certain familiar objects; the forms were thus represented by holes in the screens. The edges of the forms were softened in outline by layers of fine black gauze, which projected successively farther and farther into the holes. The screens themselves were hung upon a rigid cross-line, along which they could be silently shifted in or out of their place behind the open square. A solid screen was used, to fill the square, while the stimulus-screens were in motion. When this solid screen was removed, the colored light shone through the reducing and diffusing media and the stimulus-screen, and the faintly colored and hazily outlined form lay upon or rather within the background of neutral gray.

The stimuli were presented in a definite order: a tomato (red), a book (blue), a banana (deep yellow), an orange (orange), a leaf (green), a lemon (light yellow). The adoption of a fixed procedure was necessary, since the apparatus required the services of three experimenters, and confusion might easily arise. One experimenter had charge of the lantern-lights, colored and ground glasses, colored and colorless gelatines, and tissues. A second ran the stimulus-screens into place, gave them a very slight, slow motion during their exposure (in imitation of the oscillations of a 'subjective' image), and replaced them on signal by the solid screen. A third sat in the light room, with the observer, to give instructions, take down introspective reports, and signal to the experimenters in the dark room for the appearance or removal of a particular stimulus. The electrical signal-

apparatus was arranged on the floor, under the desk at which this third experimenter sat; as the wires were concealed, and the experimenter's hands were free, the connection with the dark room was, unless mistakes of manipulation occurred, not suspected by any observer.

The experimenter who was in charge of the lantern had an exact table of the changes required to raise the color-stimulus from a definitely subliminal to a moderately supraliminal value. This table was based upon the serial results obtained from the observers mentioned above. When the signal for a particular stimulus was given, the color-stimulus was exposed, step by step, as the table prescribed, and in a tempo that had been standardized by practice. That the observer, in reporting an image, really perceived the stimulus, at any rate in the great majority of cases, seems to us to be proved by the fact that in only one single instance, throughout the entire series of successful experiments, did an observer report an image before the stimulus was (1) supraliminal for the co-operating experimenter and (2) of such objective intensity that its perceptibility might be expected from the results of the preliminary control experiments. It may, of course, be objected that this proof is not demonstrative: the experimenter may have been suggestible, and the position of the limen may vary considerably from observer to observer. At the end of the enquiry we accordingly took control observations from several of our graduate observers; and we found (3) that, when the arrangement of the experiment was explained to them, so that they were in the position of the co-operating experimenter in the actual experiments, they invariably recognized the appearance of the stimulus at or before the point at which they had previously reported an image of imagination.

Care was needed that no sound should come from the dark room; that the forms should oscillate or flicker into view very gradually; and that no shimmer of light should show between the edges of the screens as the stimuli passed in and out. The first of the sources of error was easily avoided; mistakes in manipulation occasionally occurred, however, and were, naturally, fatal to the success of the experiment with the observer in question. They will be noted in detail in what follows. We discovered no other source of error in the experiments.

A white fixation-mark was placed on the ground glass window at the centre of the open square. The work was done, as we have intimated, in the evening and by artificial light; all preparations were carefully made beforehand, so that the observer supposed that he and the (third) experimenter were the only persons present in the laboratory at the time of ob-

servation. Instruction was made as simple as possible; the observer was merely told to fixate the white point, and to hold this fixation while he 'imagined' a colored object,—“for instance, a tomato.” He was then to describe his 'image,' if any image took shape. As soon as the description was begun, the attention of the observer was distracted from the window by some indifferent question ('What was that, once more'? or 'Quite clear, did you say'? or something of the kind), and at the same moment the experimenter signalled to the dark room for the turning out of the lantern-lamp or the swinging into place of the solid black screen.

No blank experiments were introduced, as we feared that their introduction would reveal the actually perceptual character of the induced 'image.'

Experiment I. Partly in order to rehearse the technique of the experiment, partly in order to see what would happen with wholly unsophisticated observers, our first observations were taken from three children; two girls of 13 and 14, and a boy of 10. The elder girl took the perceptions, as a matter of course, for images of imagination. The younger girl was excited and pleased by the 'images,' but had no suspicion of their perceptual character; she was astonished and chagrined when informed, the next day, of the arrangement and object of the experiment. The boy caught a flash of light (due to faulty manipulation in the dark room) early in the course of the experiment, and jumped to the conclusion that he was seeing 'shadows' cast somehow upon the screen.

Experiment II. Full sets of observations were obtained from a group of 27 observers, of whom 19 were sophomores in the university, and 8 were graduate students engaged in advanced work in the laboratory. Only 3 of these 8, however, were familiar with their furniture of images, and had worked experimentally on the topic of imagery.

Three of the undergraduate observers were ruled out, early in the experiment, by an error of technique, which was at once pounced upon and reported. The remaining 24, men and women alike, invariably mistook the perceptual for the imaginative consciousness. At the end of the series, after all the introspections had been recorded, the observer was asked whether he was 'quite sure that he had imagined all these things.' The question almost always aroused surprise, and at times indignation. Yet when asked, further, if he had ever had such images before, he would usually reply that 'he could not remember that he had; but then, he had never tried.'

It is unnecessary to quote the introspections in detail. The following remarks are taken each from the report of a different observer.

"It seems strange; because you see so many colors, and know that they are in your mind; and yet they look like shadows." "I can spread it [the color] over if I want to." "It is a pure memory, with a little effort I could move it to the wall." "It is just like seeing things in the dark; I had it in my mind." "It is just as imagination makes it." "I can get blue better, because I have been working with a blue square lately." "I can get it steadily so long as I keep my mind absolutely on it." "I can get the shading on it as I think of it; at first I think it flat, as if painted." "I can see the veining of the leaf and all." "The banana is up on end; I must have been thinking of it growing." "It is more distinct than I usually do [than the images I usually have]; but I have never tried much." "I got it; that was grand." "I am imagining it all; it's all imagination." "Feels as if I was making them up in my mind." "I get thinking of it, and it turns up."

Several times an observer remarked, towards the end of the series, that his images were something like after-images, and that he felt he could move them by moving his eyes; here, no doubt, the slight oscillation and fluctuation of the stimulus were in play. One graduate observer apologized for her 'poor imagination,' and said she could get forms but not colors; as a matter of fact, she failed to see the color of the stimulus even when it was increased very considerably beyond the ordinary supraliminal point; the forms she regarded as imaginative. Another graduate observer, who had had long experience in the laboratory and had worked to some extent with imagery, showed, both by the time of appearance of the image and by its characteristics (shape, position, size), that he was incorporating the perception in it, while he nevertheless supplied a context of pure imagery: the tomato was seen painted on a can, the book was a particular book whose title could be read, the lemon was lying on a table, the leaf was a pressed leaf with red markings on it. All the observers noted that the banana was on end, and not as they had been supposing they thought of it; yet the circumstance aroused no suspicion. Some saw an elm leaf when they had been trying for a maple leaf. The observers not infrequently volunteered the statement that they could continue to hold the image after closing the eyes.

There is, then, no ambiguity about the results. The experiment is, however, open to the objection that the observations were made, for the most part, by unpractised observers.

Experiment III. To meet this objection, we repeated the experiment, after a year's interval, with five graduate students: *K*, *S*, *T* (men), and *C* and *V* (women). All had had practice, and *T* and *V* unusually extended practice, in the observation of images.

S was for some time confused: at first he thought the

figures imaginary; then he speculated whether they might not be after-images of some sort, or akin to after-images. Finally, after the appearance of the fourth form, he remarked: "It seems like a perception, though the attention is more active than in perception; yet I feel sure that it is there, and that I did not make it; it is more permanent and distinct than an image." The permanence and distinctness were, unluckily, due to faulty technique; the signal to put out the light failed to carry its message, and the stimulus was left showing at rest.

T thought that the 'image' was rather more like an after-image than the images he was accustomed to get in daylight; but he was emphatic that it was not a perception. *K*, too, insisted that the figures were imaginative.

C took the perception for an image of imagination until a mistake of technique (with the fourth form) revealed its real nature. *V* added many imaginary details, such as 'printing' on the book, but was sure that the figures were "all imagination, of course"; "quite like those I get in the daytime—perhaps more normal"; "more strain than in a perception" (*cf.* *S*'s active attention); "could feel it formed in my mind—came right out of me"; "if I hadn't known I was imagining, should have thought it real."

The results thus confirm those of the previous experiments. And the net outcome of the work is, we think, positive and important enough to justify the time and labor spent upon its preliminaries. We find, in brief, that a visual perception of distinctly supraliminal value may, and under our conditions does, pass—even with specially trained observers—for an image of imagination. We are at a disadvantage in not being able to express our stimuli in quantitative terms. This much may, however, be said: at the conclusion of the experiments, a demonstration of the colored forms was made to a number of students in the psychological department, and to some psychologically competent visitors. Every one of these observers showed great surprise; most of them 'would, of course, take our word' for the facts, but could hardly credit them; a few remained entirely incredulous—'we asked them to believe too much.' These attitudes are sufficient warrant for the normally supraliminal character of the stimuli employed.

Historical Note. Münsterberg concludes from his experiments on the mis-reading of words "dass die im normalen Zustand reproduzierten Empfindungen unter günstigen Bedingungen von sinnlichen Eindrücken nicht unterschieden werden können" (Beiträge zur experimentellen Psychologie, iv, 1892, 22). Külpe remarks that "the limits of possible variation in these experiments are plainly restricted; and the experiments themselves cannot be considered as wholly free from objection . . . The observer's statement that he clearly saw the whole

word is hardly a guarantee that errors [of the kinds specified in the criticism] were eliminated" (Outlines of Psychology, tr. 1909, 183). Pillsbury's results, gained from similar experiments, "seem to show that the centrally excited sensation, no matter how certain of its existence we may be, possesses certain characteristic differences that distinguish it from the peripherally excited." "Letters wrongly completed were said, *e. g.*, to be of a different color from those which were actually present upon the screen; were less definite in outline; and were less stable, *i. e.*, seemed to be in motion over the word." Pillsbury adds that the method is not suited to the problem; the observer is interested in the word as a whole, not in the details of the letters (this *Journal*, viii, 1897, 367 ff.).

Külpe had made experiments on the question before 1892 (Philosophische Studien, vii, 399), and gave a brief account of them in 1893 (see Outlines, 182 ff.). They were fully reported in 1902 (Philosophische Studien, xix, 508 ff.). The observer, placed under experimental conditions, was asked to say whether he saw or felt anything, and if so what it was like, and whether he thought that it was objective or subjective: the stimuli actually given were a faint illumination of the walls of a dark room, and a pressure upon a pressure spot, with variations of interval, duration, intensity, etc. Külpe concludes that the criteria of subjectivity and objectivity are neither immanent nor universal; so that the predicates objective (referable to an external stimulus) and subjective (attributable to the condition of the observer) are always secondary and empirical determinations. We may point out that our own experiments were, in a way, the converse of Külpe's; his instruction was to report any sight or touch, and to say whether it was a matter of perception or of imagination; we asked definitely for an imagination, and sought to substitute for this, or to incorporate in it, an otherwise supraliminal perception. By thus demanding an image, we gained a further advantage; for Külpe's method does not guarantee the arousal of 'centrally excited sensations.' Külpe himself declares, *à propos* of his visual experiments: "inwiefern auch central erregte Empfindungen dabei eine Rolle gespielt haben, wage ich jetzt nicht zu entscheiden . . . Dass an der Vermengung mit objectiven Erscheinungen auch central erregte Empfindungen mitgewirkt haben, ist wahrscheinlich" (P. S., xix, 528, 533). Our experiments evidently called up images in all those cases in which the perception was supplemented (*lemon on table*, etc.); while in the other cases, whether or not an image was called up, there was a presentation which simulated and was identified with an image—and this under the most careful scrutiny and with sustained attention. For the rest, it is worth while to note that the differences between perception and image of imagination found by Pillsbury are all of the kind described by Külpe as secondary or empirical.

Washburn, in her study of subjective colors and the after-image (Mind, N. S., viii, 1899, 32), remarks that "perception and idea differ ultimately only in the manner of their production. . . . Where the intensity of the peripheral process is reduced to a minimum, the resulting conscious state is seen to be practically identical in character with that produced by central excitation." And there are many other experimental indications of the introspective identity of sensation and image: the nature of the visual images in Galton's 'highest class'; the possibility of securing an after-image from a 'mental image' (Downey); the probability of the intervention of images in certain normal illusions (Seashore and others); the facts of the memory after-image, of synesthesia, of hallucination. It is not our intention to review these topics, or to enter upon the question of the cerebral seat of sensation

and image: all this would carry us too far afield, and the time is not ripe for full discussion of the image of imagination upon an experimental basis. We wish only to remind the reader that our experiments have a positive, if fragmentary, experimental background.

Much has been made of Lotze's statement that "the idea of the brightest radiance does not shine, that of the intensest noise does not sound, that of the greatest torture produces no pain" (Outlines of Psychology, tr. 1886, 28; Medicinische Psychologie, 1852, 477 ff.; Microcosmus, i, 203 f.). Titchener has recently remarked that a view of this sort, in so far as it depends on an intensive comparison of sensation and elementary image (a thing impossible, for most persons, in the case of pain), is probably due to the intercurrent error of the stimulus error (Experimental Psychology of the Thought-processes, 1909, 267); and experiments now (March, 1910) in progress in the Cornell laboratory serve definitely to confirm that hypothesis. Ziehen's dictum that "the ideas of the slightest rustling and of the loudest thunder exhibit no difference in intensity whatever" (Introd. to Physiol. Psychology, 1895, 154; Leitfaden, 1906, 136) is flatly untrue if the rustling and the thunder are reproduced in kind; if, that is, images of auditory imagination replace auditory perceptions; for auditory images, as will be shown in a later communication from the laboratory, possess the attribute of intensity. The very fact that statements such as these can be made witnesses to the extreme need of experimental observation.

In general, of course, the experimentalists incline to the view that there is no intrinsic difference between sensation and image. It is not our intention, again, to review this topic; the review would be premature. We refer only to A. Meinong's classical discussion in the Vierteljahrsschrift für wissenschaftliche Philosophie, 1888-9, Nos. 11, 12, 13; and to Münsterberg, Die Willenshandlung, 1888, 138 f.; Grundzüge der Psychologie, 1900, 311; Külpe, Outlines, 85, 183 (it is to Külpe, so far as we are aware, that psychology owes the terms 'peripherally excited' and 'centrally excited sensations'); Stumpf, Tonpsychologie, i, 1883, 260, 375 f.; Wundt, Human and Animal Psychology, tr. 1896, 14; Outlines of Psychology, tr. 1907, 31, 39; Physiological Psychology, i, tr. 1904, 13 f.; Pillsbury, Attention, 1908, 95; Titchener, Elementary Psychology of Feeling and Attention, 1908, 337; Text-book of Psychology, i, 1909, 198 f.

§ II. KINÆSTHETIC ELEMENTS IN IMAGES OF IMAGINATION AND IMAGES OF MEMORY

Our next concern was to bring the image of imagination into relation, for purposes of introspective comparison, with images of memory. As a stimulus to the image of imagination we could think of nothing more promising than the spoken word. There is, truly, no guarantee that the spoken word will arouse these images; but it seemed practically certain that, if a fairly large number of experiments were made, the two types of image would be found among the results. The observers were accordingly seated in a dark room, and instructed to give themselves up to the visual imagery evoked by the word; they were not in any way to control or regulate their imagery, but were to give it rein, passively and as association determined. No classification *a priori* was attempted.

It soon appeared that a good proportion of the images thus

aroused were of two sharply different kinds. There were, on the one hand, images of recognized and particular things, figuring in a particular spatial context, on a particular occasion, and with definite personal reference; and there were, on the other hand, images with no determination of context, occasion, or personal reference,—images of things recognized, to be sure, but not recognized as this or that particular and individual object. The former were evidently 'images of memory;' the latter, both by positive and by negative character, were 'images of imagination.' In other words, we have in our results a rough and ready criterion of the two types of image: memory being distinguished by particularity and personal reference, and imagination contrasting with it by lack of particularity (in the sense of a particular sample, placed and dated) and absence of personal reference. There were, naturally, a fairly large number of intermediate forms (images with personal and place references, but unfixed in time; images with personal reference, but neither temporal nor spatial context; images with context but no personal reference). The classification of these under the one or the other of the two main rubrics would have been possible, from the records, although it would have left a margin of uncertainty, aside from that due to the possibility of an incomplete introspective account. Fortunately, however, we had no need to attempt it, as the clear-cut cases were sufficiently numerous for our purpose.

One of the first things noticed, as we sought to analyze the images, was the presence of certain kinæsthetic factors in the image of memory which were not traceable in that of imagination. Sensations of eye-movement were by far the most obvious; memory appeared to imply roving movement of the eyes, while imagination implied steady fixation. The indications were that other and more general kinæsthetic differences obtained, but this, of eye-movement and fixation, was the most noticeable. In view of the important part played in theory by the kinæsthetic factor, we decided to put this discovery to the test of experiment.¹

The observer sat in a dark room, his head supported in a

¹ We naturally think of the seer and dreamer as rigid, in a fixed stare; and we know that the effort to remember sends our eyes wandering over walls and ceiling, as if we hoped somewhere to find a cue to memory. Cf. F. Meakin: Mutual Inhibition of Memory Images, *Harvard Psychological Studies*, i, 1903, 244; C. S. Moore: Control of the Memory Image, *ibid.*, 296; J. W. Slaughter: Behavior of Mental Images, this *Journal*, xiii, 1902, 548; F. Kuhlmann: Analysis of the Memory Consciousness, *Psychological Review*, xiii, 1906, 338 f.; E. Murray: Peripheral and Central Factors in Memory Images, this *Journal*, xvii, 1906, 241; Külpe: *Outlines*, 1909, 187; etc.

headrest, and his left eye screened. The right eye fixated a luminous (phosphorescent) spot 1.5 m. distant. Four other and similar luminous spots, at the same distance from the eye, were placed within and near the upper, lower, right and left margins of the blind spot of the fixating eye. A sixth luminous spot was placed to the right, just beyond the limit of the field of vision. So long as fixation was continued, or the fixating eye moved only within certain narrow boundaries, the outlying lights remained invisible; so soon as the movement of the eye attained a certain extent, some or all of the outlying lights flashed into the field.

The observer was required, first, to gain such control of fixation that the five outlying spots remained permanently invisible when the head was in position; there was some difficulty with all observers, but it was overcome by practice. In the experiments themselves, a signal was given, and then the word was called out to the observer, who signalled (by a minimal hand-movement, which did not at all disturb fixation) the appearance of an image. Careful and detailed introspections were taken of the course and character of the image, and a note was made of the appearance or non-appearance of outlying lights in the field while the image was in course. The time elapsing between the giving of the stimulus word and the arousal of an image was recorded by the experimenter; but no use will be made of this determination in the present paper. The observers were left altogether in ignorance of the object of the work; they soon discovered that the movements of the eye were in question, but no one of them, so far as we are aware, realized that we were in search of a distinction between image of memory and image of imagination. These terms were carefully avoided; the experimenter spoke only of 'image.'

Obvious sources of error were: the limit of function of the apparatus, the possibility that the observer should forget the appearance of outlying lights in his anxiety to do justice to the image, and the occurrence of intermediate and dubious forms of image. In so far as the apparatus allowed of slight movements, without sight of the lights, its limitation would serve to increase the number of observations of 'imagination with no movement,' and to reduce that of observations of 'memory with movement.' Yet the movement that would bring in the lights was very slight, and the movements involved in the memory image were gross; so that we do not ourselves attach weight to this source of error. The second we must accept; though the training and attention of the observers may be supposed to reduce it to a minimum. It appeared, without suggestion from the experimenter, in the introspections of the sentence-experiments (Experiment VI), and is there allowed

for. The third we avoided by discarding all equivocal results in our final calculations. Lest, however, this procedure should seem arbitrary and perhaps unfair, we sought in every instance to make a distribution of the doubtful cases, as impartially as possible, to the two main categories (an example is given in the auditory experiments: Experiment VIII); and in every instance the outcome of the combination told for and not against our main thesis.

Experiment IV. The observers, Messrs. Geissler, Tsanoff, and Williams, and Misses Clarke, Day, and Rand, were all graduate students; only *D* and *T* had had practice in the observation of images. After ruling out all equivocal images, 5 cases of images of printed words, and 16 cases in which mistakes of technique occurred, we had 426 experiments of which 212 gave images of memory (in the sense explained above) and 214 gave images of imagination. These results were somewhat irregularly distributed among the observers, but no observer reported less than 20 images of each kind.

Of the 212 memory images, 191 (or 90%) involved eye-movement, to the extent that outlying lights came into the field of vision; of the 214 images of imagination, 146 (or 68%) showed no evidence of movement. Of the 68 images of imagination for which movement was recorded, 12 were images of animals conceived as running swiftly across the field of vision, and 17 were images of greater extension than could be compassed by the resting eye,—images of a stretch of landscape, or of the side of a near and large building. If we rule these out as equivocal, we have only 21 anomalous cases of memory (no movement) and 39 anomalous cases of imagination (movement recorded).

Experiment V. In a second experiment, the experimenter selected the stimulus words with a view to the avoidance of very extended images, and of images of moving animals. Minor improvements in technique (size and permanence of the phosphorescent spots) were also effected. In spite of preliminary practice, some of the observers had reported a certain difficulty in the presence of the fixation spot; the peripheral stimulus seemed to interfere with the formation of the image. At the conclusion of the first experiment, however, this difficulty had been overcome; so that, on the whole, better results might now be expected. The observers were as before.

Of 227 memory images, 218 (or 96%) involved eye-movement; of 165 images of imagination, 122 (or 74%) showed no evidence of movement. But, in spite of precautions, 30 of the 43 refractory cases of imagination were cases in which the observer imaged an extended scene. Of the 122 cases without movement, 6 were also of extended scenes; but here no estimate could be made (as was ordinarily the case) of the distance of

the scene from the observer. In 10 of the remaining 13 cases of imagination with movement, the observer reported a fatigue of fixation before the appearance of the image. There were no other discoverable sources of error in the experiment.

Experiment VI. It did not seem probable that the weight of evidence could be increased by further experiments of this kind. There was, however, a possibility that the use of the single word as stimulus induced an artificial or abnormal steadiness of fixation, which would not exist if a series of images of imagination were passing through consciousness. We accordingly turned to serial stimuli; we employed sentences, bits of nursery rhyme, passages from Shelley's *Cloud*, Lanier's *Song of the Chattahoochee*, Kipling's *Chant-Pagan*. These were read slowly and distinctly by the experimenter; the observer signalled the appearance of an image, and the corresponding word was noted by the experimenter; introspections were taken at the end of the reading. The observers were *R*, *T* and *W*.

Of 94 memories, 68 (or 72%) involved eye-movement; of 269 imaginations, 243 (or 90%) showed no movement. In 10 cases (thrown out) the observers could not remember whether or not the lights had flashed into the field of vision. In the other cases, they felt fairly certain of their report; though the affective state aroused by the stimuli was at times so strong, and the interest in the images so intense, that peripheral stimuli might, perhaps, pass unnoticed. We must leave this possibility undetermined.

As might have been foretold from the imaginative character of the stimuli, there is a great preponderance of images of imagination. In many cases, an existing image or fusion of existing images served to form a context for a newly entering image, which still remained positively an image of imagination; the ready-made context did not make it over into an image of memory. Sometimes there was, in these cases, a sort of personal reference, but it was radically different from the reference of memory; the observer would 'see himself there' rather than feel himself there; he seemed detached from himself, filling "a blank space which represented himself;" "there was somebody in the boat which [*sic*] I supposed was myself." By this time, indeed, the observers had frequently remarked on a conscious difference in types of images, noting that "sometimes the context seems the important thing, and sometimes the image itself," and that a 'particular' image seemed to be of a different mental order from an unreflected image. It was time, then, to transcend the preliminary and non-psychological criteria that we had so far relied upon, and to seek to ascertain the introspective basis of this difference.

Experiment VII. Comparatively few observations were taken in this Experiment, as the introspective reports were made as full as possible. The observers were *D*, *T* and *W*. The word method was employed.

Of 39 memories, 35 (or 90%) involved eye-movement; of 61 imaginations, 53 (87%) showed no movement. Other determinations were as follows:

100% of imaginations reported that fixation was apparently necessary before images could be secured;

75% reported lively affective processes;

92% reported great vividness of imagery;

93% reported a 'feeling of personal detachment;'

41% reported a mood of strangeness or novelty.

Introspective comparison of the two types of image brought out the fact, for all observers, that "the difficulty in holding the fixation is greater with the particular than with the generic images ['generic' is a term employed spontaneously by the observer quoted]. The latter seem to require fixation anyway, it seems to build them up before your eyes as you gaze steadily, while for the others you seem to want to look around, or to be free in some sense, and the fixation-point seems to hamper you."

Experiment VIII. It was still a question, however, whether the kinæsthetic factor present in the process of memory were peculiar to visual imagery; and whether it were a responsive movement, or a preparatory movement of adjustment of the sense-organ involved. The latter question might also, in principle, be asked of fixation. We accordingly arranged an Experiment for testing the laryngeal movements that are associated with inner audition. The observer assumed a semi-recumbent position on a couch, the inclined portion of which supported the upper part of the body and the head. We hoped through this position to avoid head and body movements, as well as general muscular strains and fatigue. The Verdin laryngograph, rigidly supported, was applied to the larynx, and so adjusted that any movement which fell within its range of sensitivity could be recorded on a kymograph. Record was made, on the same surface, of the appearance of auditory images in the mind of the observer. The arm and hand signalling these appearances were comfortably relaxed, and preliminary tests were taken to make sure that the signal movements did not change the laryngographic tracing.

The observers were *D*, *W* and Mr. Nakashima; all graduate students. Three other persons who offered their services were rejected, since they were unable to obtain auditory images. Another had good images, but gave no record of laryngeal movement under any conditions; he had been a singer as a

boy, and had overstrained his throat,—a fact which may account for this absence of appreciable movement. The preliminary work with these rejected observers was useful as affording practice in the manipulation of the laryngograph. The word method was employed.

Of 155 memories, 84% (minimum 80%, maximum 90%)¹ involved movement; of 214 imaginations, 91% (minimum 90%; maximum 96%) showed no movement. There were 56 doubtful or equivocal cases. We have sought, on the basis of the very full introspections, to distribute these as fairly as possible to the two main categories; with the result that memories with movement rise to 86%, and imaginations without movement to 94%.

The averages here, however, are a little misleading. For *D* and *W*, the most practised observers, at no time recorded movement with images of imagination; and *D* gave but one case of memory without movement, while *W* reported only 6 such cases. *D*'s record was especially clear, the movement showing conspicuously in the tracing with the memories, and being as conspicuously absent with the imaginations.

This Experiment was repeated, with like result, after an interval; there were, again, three observers. We do not think it necessary to report the details.

Experiment IX. One of our observers, Miss de Vries, had mentioned that strong olfactory images were of common occurrence in her experience. Experiments were therefore made, again by the word method, in this department of imagery. The observer lay on the couch, in the position described above, with eyes closed, and signalled the appearance of an image. The experimenter carefully watched the observer's face for signs of movement. Introspections were taken after the image had run its course.

Of 56 memories, 96% involved movement of the nostrils, and 86% very definite movements, a sniffing in and out of the nostrils and a jerking of the head. Of 57 imaginations, 46 or 80% gave no perceptible movement. Of the remaining 11, 9 involved only a very slight twitching of the nostrils, just observable by the experimenter; and 5 of the corresponding images had a local visual setting, although this was not definitely recognized.

We lay no great weight upon these results, though they are evidently in accord with those obtained with vision and audition.

¹The record was delicate, and not always easy to read or interpret. Hence we give the possible minimum and maximum, as well as what seems to us and to our assistant readers the fairest percentage.

No experiments were made upon the imagery of touch.

If we summarize the results of the foregoing Experiments, we have the following:

No. of Obs. Memory Imagination		Imagery	Percentage of Memory and Movement	Percentage of Imagination and No Movement
572	709	Visual	89.5	79.5
155	214	Auditory	84	91
56	57	Olfactory	96	80

As they stand, the figures are significant enough. But it must be remembered that we have, throughout, given the observation the benefit of the doubt; we have made no distinction between stages of practice, we have allowed full weight to discrepant observations which nevertheless could readily be accounted for (running animals, etc.). In other words, our figures are as low as they can be made, and might easily and without undue pressure of interpretation have been made higher.

SIII. AFFECTIVE FACTORS IN MEMORY AND IMAGINATION

It would be hasty to conclude, from the foregoing Experiments, that the imaginative consciousness is constituted as such by a typical distribution and proportion of kinæsthetic elements, and by this alone. Emphasis has been laid, in many quarters, upon the affective component in imagination, and it is necessary that the affective theory be brought to the test of experiment.

We thought, at first, of having recourse to the method of expression. But we gave up this idea: partly on account of the perplexity in which the method is itself involved, partly because we did not see how, at its best, it could help towards a solution of our problem. The questions at issue are whether there is a qualitatively characteristic mood which informs consciousness in memory and in imagination, and whether—granted that the moods exist—the imaginative consciousness is more strongly, more markedly affective than the memorial. We knew from Experiment VII that the word method could bring out reports of mood, and we accordingly adopted it, though in slightly modified form, in the present case.

Experiment X. We selected two observers who had vivid visual imagery: Miss de Vries, of whom we have already spoken, and Dr. Pyle, an assistant in the laboratory. Both observers were familiar with the standard investigations of mental imagery; and both had had practice in this field of observation. The new experiments were made in diffuse daylight: the observers sat at about 1.5 m. from a buff-colored blank wall; their eyes were open, but fixation was not prescribed. The general instruction was that they should report upon the mood accompanying or infusing their images; and that (if this procedure were of any help to them) they should

compare images of different types, in order that the moods should be intensified 'by contrast.' The instruction to compare the images set up, in both observers, a tendency to the alternation of imagination and memory, though this tendency was oftentimes cut across by the intrinsic suggestion of the stimulus. The results were similar for the two observers, and need not be separated.

Of 103 imaginations, 72% involve some mood of surprise. This was variously described as wonder, novelty, strangeness, queerness, unusualness, weirdness, fantasticalness, creepiness, peculiar discomfort, and lack of ordinariness. The imaginations not thus accompanied were almost always images of commonplace objects, which had been a part of the ordinary mental furniture of the observer; that is, they were habitual imaginations.

There were 154 memories. In many cases, the feelings and emotions connected with the remembered event were so strong that it was hardly possible to determine whether any affective coloring attached to the image as image. In all other cases, both observers found a recognitive mood; and both expressed the opinion, at the end of the series of observations, that a glimpse of this mood could be caught, by alert introspection, at some stage or other in the course of every memory image.

We may say then that, under the conditions of our experiments, surprise or novelty is the characteristic mood in imagining, and recognition or familiarity the characteristic mood in remembering. On the question of relative intensity, the observers thought that the mood of imagination appeared the stronger simply because it is not interfered with by other emotions or moods. The image of imagination is new and strange, and that—so far as our experiments go—is all; the image of memory is familiar, but it is also, as a rule, affectively colored in its own right. So the recognitive mood is likely to be swamped. When, however, the memory was indifferent, it seemed to be as strong as the mood of surprise in the corresponding imaginative consciousnesses. And had we succeeded in arousing images of imagination in which the observer took an inventor's pride, or which inspired him with disgust, then (our observers thought) novelty would have weakened to the ordinary level of familiarity.

We attempted no analysis of the moods of surprise and recognition; the conditions were evidently unfavorable to such work, and we desired only to find out what was present, in the imaginative and memorial consciousnesses, that could be considered as a characteristic affective formation. The mood of recognition is genetically, in all probability, a very degenerate form of the emotion of relief, of fear unfulfilled (Titchener, *Primer of Psychology*, 1900, 191). The mood of surprise, of novelty, of strangeness is apparently a weakened form

of our instinctive fear of the unknown; it shows its derivation, with what was to us an unexpected plainness, in its unpleasant nature: the observers speak of creepiness, weirdness, peculiar discomfort.

Instances of the suppression of novelty by another mood (pleasure in the completed image, charm) will be given later. It is probable that the generally unpleasant character of the mood of novelty, in the records of the present experiments, was due in part to the fact that the observers were dealing, not with a continuous imagination, but with single, detached images, and in part to the fact that these images were suggested to them by words coming in from the outside rather than by the course of their own consciousnesses: *cf.* Titchener, *op. cit.*, 207.

§IV. THE IMAGE OF MEMORY AND THE IMAGE OF IMAGINATION COMPARED

We have now to discuss in detail the introspective differences between the image of imagination and the image of memory. The initials *P* and *V*, in the following paragraphs, refer to the observers in Experiment X (daylight images). For other observations we rely mainly upon Experiment VII (dark room images).

I. FIXATION

Imagination. Both *P* and *V* noted, without instruction given, that fixation was necessary if an image was to appear at all. *V* could never move her image, though she could duplicate it at some other point on the wall after renewed fixation. She reported that she could see the original image in its original place, by indirect vision, while she held the new image in direct vision.¹ *P* thought that he could move his

Both *P* and *V*, again, found that the image nearly always image, but was in fact never able to follow it in passage, and said of his own accord that he was not sure it was not a new image, built up after renewed fixation.²

¹*Cf.* Moore: *op. cit.*, 302.

²All of our observers who could obtain images of visual imagination expressed their belief, when questioned, that they could move their images at will; we have noticed an instance under Exp. II. It seems, from the experience of *P* and *V*, that such movement is more probably a shift of fixation followed by the development of a new image; but the point demands further investigation. *Cf.* E. B. Huey, *Psychology and Pedagogy of Reading*, 1908, 32 ff.

P found that his images invariably tilted in position when he tilted his head; with great effort of attention he could sometimes right them. The same observer, who suffered from imbalance of the ocular muscles, was at times unable, towards the end of the experimental sitting, to hold a steady fixation; his images then 'rolled around' or 'turned somersaults.'

Both observers reported that they could follow the lines (*e. g.*, the curved lines of a coiled snake) within the image, as one can follow the lines in a stereographic perception.

remained unchanged as long as it was visible at all; like a perception, it stood still to be scrutinized, though it yielded no more detail than at the first glance. Occasionally, when the first appearance of the image was incomplete or otherwise unsatisfactory, transformations would be observed in the defective parts, or the entire image would flicker out, and a new one would take its place.¹

Usually the image was on, or at the exact distance of, the wall. Sometimes, especially for *V*, it was nearer, and induced a "cross-eye feeling"; the point of fixation could then be roughly determined by passing the finger back and forth through the image and noting double-images. If the object imaged were too large for the field of vision at the distance of the wall, the perception of distance was rather uncomfortably disturbed; the image was 'felt' to be at the distance of the wall, but was of a size that the imaged object should have had at a greater distance. The wall, under these conditions, dropped out of consciousness, or remained only to color the atmosphere surrounding the image.

A series of 20 tests gave the average duration of *V*'s images as about 90 sec. The maximal duration noted was nearly 180 sec. If an effort was made to hold the image to the bitter end, it grew smaller and less distinct, receded in space, and finally faded out. Otherwise, it simply ceased to be there.

We may add the general statement that both *P* and *V* frequently reported absence of bodily movement, and lack of diffused kinæsthetic sensations, especially after comparing these images with the images of memory.

Memory. These images did not involve a steady fixation, but rather a definite eye-movement; the observer was conscious of seeking them in a definite direction.² The image appeared with a determinate orientation to the observer, which might, however, differ widely from that of the original experience. When once the image had appeared, the observer could very rarely, and then only with the greatest effort of attention, get it with any other orientation; if he succeeded, the image

¹ Several experimenters have reported oscillation or fluctuation of the image. *V* sometimes noticed fluctuations, at first of a period of some 40 sec., and then of increasing frequency. Fluctuation seems to vary not only with the individual, but also with the condition of the individual. Cf. Moore: *op. cit.*, 295.

Both observers found that the image flickered with winking, and that if the eyes were closed it usually disappeared or, at best, remained for a very short time.

² This point has been noticed by many investigators: Fechner, Kuhlmann, Meakin, Moore, Murray, Slaughter.

was a 'ghost image,' barely perceptible and very difficult to hold. Neither *P* nor *V* was able to move the image.¹

The image was fleeting and instable; it could not be deliberately scrutinized; it stayed for 10 seconds at most. The strong tendency was to move incessantly from part to part of the image-content.²

As a rule, the images appeared to be some distance away, often approximating the distance of the remembered object from the observer at the time of observation.

The images involved a good deal of general kinæsthesia. *V* could see the pail, which on the remembered occasion she swung in her hand, only by holding her arm up before her eyes as she sought to form the image. So far as introspection could determine, the movements involved in memory were weaker duplicates of those involved in the remembered experience.³

2. VISUAL CHARACTERS

Imagination. The images were apt to take on the illumination of their surroundings. In the dark room, they were dusky, apparently illuminated by the phosphorescent fixation spot. Sometimes this spot became an integral part of the image: a candle in a window, a lantern in a cave, etc. In the daylight they were much lighter; closing the eyes darkened them, and tinged them with red. They often took on a fringe or halo of the color of the wall.

V incidentally noted a distinct after-image succeeding the image of a fire. After this observation, *P* and *V* made special tests of the arousal of after-images. *P* was never certain that he obtained an after-image, though at times he thought he had a vague negative image. *V* obtained several fairly distinct negative after-images.

On certain occasions, when the image was very near, *V* tried the effect of alternate monocular observations, and found that the image changed precisely as perception changes.

Memory. The images did not take on the illumination of their surroundings; even in the dark room they retained their daylight character, if the originals had been daylight experiences.⁴ The fixation spot in the dark room never became

¹ *P* found that it did not alter its position when he tilted his head. The image did not, for either observer, flicker with winking, nor did closing the eyes banish it.

² Cf. Murray: *op. cit.*, 231; Slaughter: *op. cit.*, 531.

³ Cf. G. M. Whipple: this *Journal*, xiii, 1902, 259 f.

⁴ We had no objective control of the dullness or brightness of these images; for that, recourse must be had to simpler modes of image (Bentley, this *Journal*, xi, 1899, 42 f.). The point here is that the imaginations were dark in the dark and light in the light, while the

a part of the image. In the moderate diffused daylight of the laboratory, the observers felt themselves blinking in the glare of a remembered sunny path. Illumination was not affected by closing the eyes. There was no fringe from the color of the wall. No after-image was obtained from a memory image.

The visual imagery was scrappy and fleeting. The observer would be aware that there were large blanks in the visual picture, but the instant he turned his attention to any particular blank it was no longer empty, but filled by a bit of imagery. Trial was made in vain to trace the boundaries of the blanks, or of their visual fillings. There was a great deal in consciousness (organic, kinæsthetic processes) which the observer could convert at any moment into visual contents. The visual contents themselves were filmy; 'colorless etchings,' 'rough outlines;' they fused so intimately with the entire mental complex that the observer was forced, again and again, to remark "I don't know what I see and what I don't see." The images of imagination were oftentimes, on the contrary, very highly colored.

3. AFFECTIVE AND ORGANIC ELEMENTS

Mood. The mood of imagination is, as we have seen, that of surprise (unfamiliarity, novelty, strangeness); the mood of memory is, patently, the recognitive mood. In the case of habitual imaginations, of what one of our observers termed 'generic images,' there was either no affective setting or a very weak mood of familiarity.

We were especially interested to discover feelings of reality (or unreality) and activity (or passivity). Both types of consciousness usually excluded any thought of the image as an image; the image engrossed the attention, and the experience was real. However, we gained some hints—more could not be expected—of the basis and contents of the feelings mentioned.

In the case of imagination, the observers sometimes remarked that they "felt as if it had been a dream," that they had "lost time." Here was a feeling of unreality. More often they would say: "it seemed more real than reality," "not so real as the memory, of course, if you consider it as an experience, but more really there, real as shadows are real," "the imaginary rabbit was more real than the memory rabbit, more rabbit, and more there." Here was a feeling of something like perceptual reality. Again, "it had objective reality; not as if I had anything at all to do with it." Here was a feeling of independent reality, of the detachment of the image from the observer.

The feeling of perceptual reality seemed to be due, directly, to the sensible characters of the image; its intensity, definite color, duration, stability, steady position in a definite plane, suddenness of entrance when looked for, its wholeness and self-containedness as compared with the image of memory. The observer "took it as if it was an ob-

memories were dark or light, independently of surroundings, according as they pictured darkness or lightness in the original experience.

ject to be looked at," he "stood off and watched it." The feeling of perceptual reality appeared to be the conscious concomitant of the attitude of contemplative vision; steady fixation, with general muscular relaxation. The feelings of unreality and of independent or detached reality were based—the former, it seemed, exclusively, the latter principally—upon the lack of kinæsthesis. The observers often remarked that they "had done nothing about the image," that they had not responded to it as they would have done to a perception. This was one reason for the difficulty of distance localization; not only was the observer aware that he got, *e. g.*, an entire person into the field of vision, as he could not have got a real person at the given distance, but he was also disturbed by the absence of any kinæsthetic cue or bodily reaction of his own, by which he might gauge the distance. The dream-character may, then, be ascribed to the lack of kinæsthesis. The feeling of detached reality is due in part to this lack, and in part to the perceptual character of the image itself.

The feeling of reality that attached to the memory image was of a different kind; it was the feeling of real occurrence in past personal experience. The observers identified it with the recognitive mood, together with the conscious concomitants of movements; movements of adjustment, of response, of imitation. These movements fairly abounded; the observer was not now a spectator of a show, but a responsible participator in the experience.¹

The conditions were not fitted for anything like an exact analysis. We add only that all observers were clear that the feelings of reality and unreality were complex, and that they would analyze into organic and kinæsthetic factors.

The image of imagination was not often accompanied by a feeling of activity; usually it "just came of its own accord." Sometimes, however, there was a feeling of exhilaration, of enhanced mental activity. On a few occasions (not invariably) we were able to connect this feeling of mental activity with the travelling of the point of regard over the lines of the image, and its 'active' examination in detail. The memory image, also as a rule, came of itself, and was receptively, passively taken; though at times there was a feeling of restless activity, always (so far as we noted) accompanying movements of imitative exploration. But the feeling of activity was not, in these experiments, at all a marked feature either of the imaginative or of the memorial consciousness.

Organic Factors. In the case of imagination we find, besides the organic concomitants of concentrated attention, a good deal of empathic sensation. With an image of a bunch of grapes the observer spoke of "a cool, juicy feeling all over;" with a parrot, of "a feeling of smoothness and softness all over me; not tactual [*i. e.*, not cutaneous];" with a fish, of "cool, pleasant sensations all up my arms; slippery feeling in my throat; coolness in my eyes. The object spreads all over me and I over it; it is not referred to me but I belong to it;" with a bowl, of "organic sensations in chest; cool object in

¹ On one occasion the observer *V* had side by side a pure fancy image and an indefinitely recognized but still distinctly memorial image of memory. She was able to look several times from the one to the other, and to compare both the images themselves and her attitude to them. She reported that "the two moods were strikingly different; it would be impossible ever to bring the two images together; both were real, but it was as if the reality belonged to two different thinking organs."

and against it;" with an apple, of "cool juicy round feeling in myself;" with a rose, of "nothing except sensations of pink; I seem to swim around in it and it went all over me." Intropections of this type were very frequent, though not always as explicit as those quoted. Frequently the observer called out, after the image had run on for a few seconds, "there! I seemed to jump into it; went all over me." Sometimes, however, the organic sensations were present from the first.

While the image of memory brought with it into consciousness many organic factors, these were always of an imitative sort, factors that had been concerned in the original experience. The kinæsthetic and organic elements were, in very many cases, at least as important as the visual imagery itself; we have quoted an observer to the effect that he was doubtful as to what he saw and what came from other cues. The whole organic effect was radically different in the two types of consciousness.

4. TEMPORAL COURSE

The images of imagination appear more quickly, more suddenly, and more as a whole, than the images of memory. They persist longer, and are far less changeable during their course.

These statements, in their relative form, hold for all observers, in spite of considerable individual variation.

5. STATE AND ARRANGEMENT OF CONSCIOUSNESS

In imagination, the attention is very narrowly focussed; there is a close resemblance to the hypnotic consciousness. The absence of the large motor responses eliminates much of the ordinary perceptual consciousness. We have as a result great clearness and intensity of the image, its spatial isolation in a surrounding haze, and the affective and organic concomitants noted above.

The word 'charm' was sometimes used to express the feeling of pleasurable excitement which went with this semi-hypnotic state. "It had the charm of a dream; felt like a dream." "It does not feel normal, though it feels real." "It is prettier than a rose ever was," "redder than wine ever was," "greener than any grass," "more brilliant than emeralds," "I feel myself through it as through a fog; the lights and shades help, and give it charm." "It is clear, though in a dim light;" "clear as an image but not as an existence;" "the only thing in consciousness;" "I became the image;" "one part stands out very clear; beyond it shades off into a fog or haze;"¹ "it looks as things look when you turn your head upside down." All these reports point to a high level of attentional clearness.

In memory, on the other hand, attention is wandering and

¹Cf. W. Lay: *Mental Imagery*, 1898, 3.

diffused. It hurries to a number of diverse factors, and rests on none. Tactual, auditory and visual images combine with organic sensations into an instable flux, sent into many channels by as many different cues. The visual image flits through consciousness, now this and now that feature of it rising to clarity and claiming interest, but all passing so swiftly that introspection is difficult. In general, the visual image is flat and thin, without much perspective or light and shade, and without the peculiar vividness of coloring that we find in the image of imagination. It is so entangled with the self-feelings and with kinæsthetic bits of spatial direction and arrangement, that the observer is at a loss to know what he sees, while he knows that he is conscious of much more than he sees; he is conscious of an underlying fixity of occurrence, of a total setting in which the visual images sink and are lost, of a breadth of context in which he can move at will, converting into visual terms this or that detail, as his interest prompts. All this is given in attitudinal terms, with definite fragments of imagery, or kinæsthesia, or some organic complex, rising in quick succession to the level of clearness.

The patterns of the two consciousnesses are thus essentially different. In imagination there is a permanent narrowing of consciousness, with inhibition of all irrelevant associative material; in memory, consciousness is as it is in the ordinary waking state as contrasted with hypnosis, a formation that now narrows and now broadens, liable to the irruption of any chance association. The great difference between memory and perception derives from the elimination of sensations of special sense, and the consequent predominance of kinæsthetic and organic factors.

SUMMARY

We have used the phrases 'image of memory' and 'image of imagination' to denote, not the elementary image-process that is co-ordinate with sensation, but a complex formation, of the same level as the perception.

(1) We find that, under suitable experimental conditions, a distinctly supraliminal visual perception may be mistaken for and incorporated into an image of imagination, without the least suspicion on the observer's part that any external stimulus is present to the eye. The perception may be of such definiteness that instructed and competent observers, in presence of it, have declared our results 'incredible' and have pronounced the stimulus 'ridiculously real.' Yet there was not one uninstructed observer who discovered the deception for himself. It follows that the image of imagination must have much in common with the perception of everyday life.

(2) For preliminary purposes, images of memory may be distinguished from images of imagination as having particularity and personal reference.

(3) We find that, in the great majority of cases, memory images of sight, sound and smell involve gross movements of eyes, larynx and nostrils, while the corresponding imaginations involve no such movements.

(4) A detailed comparison of visual images of memory and of imagination brings out the following differences: memory involves eye-movement and general kinæsthesia, imagination involves steady fixation and lack of general kinæsthesia; memory images are scrappy, filmy, and give no after-images, while images of imagination are substantial, complete, and sometimes give after-images; the mood of memory is that of familiarity or recognition, intrinsically pleasant, the mood of imagination is that of unfamiliarity or novelty, intrinsically unpleasant; memory implies imitative movement and the correlated organic sensations, imagination implies kinæsthetic and organic empathy; memory images arise more slowly, are more changeable in course, and last less long than images of imagination; memory implies roving attention and a mass of associative material, while imagination involves concentrated and quasi-hypnotic attention with inhibition of associations.

We thus reach the general conclusion that the materials of imagination are closely akin to those of perception. Popular psychology looks upon memory as a photographic record of past experience, and regards imagination as working with kaleidoscopic, instable, undependable materials. Precisely the reverse appears to be true. The image of memory is stable and fixed in meaning, in reference; but it is exceedingly instable as conscious content. The image of imagination is the photographic record, a stable formation that stands still to be looked at. This state of affairs seems, indeed, after the event, natural enough. It is just because the memory image is instable, liable to all sorts of interchange, suppression, short-cutting, substitution, telescoping, that it is psychologically available for memory; that a mass of past experience can be packed into small representative compass. And it is just because the image of imagination is stable and unchanging that it is psychologically available for the artistic purpose, for constructive embodiment. If an image could not decay, we should have but little memory; if an image could not persist, we should have but poor imagination.

It is now a question whether the stability and vividness of the image of imagination are due simply to its kinæsthetic support, to the immobility of the organ of sense, or whether peripheral sensory excitations are actually involved in it.

We incline to the latter view, though we regard the kinæsthetic support as a contributing factor.

Finally, we wish to emphasize the point that our results, positive as they are, hold at present only for the conditions under which they were obtained and for the observers upon whose introspections they rest. Any attempt at generalization would be premature. We have found that imagination is distinguished from memory by sensory vividness; but it may be that this vividness is not essential to imagination, and in any case it does not mark off imagination from pictorial thought. We have not found that imagination is in general more markedly emotive than memory; but, under other circumstances, this may prove to be the case. We have not found that imagination implies a plan, a voluntary synthesis; yet it may do so in other cases. We have dealt only with two complexes, of a low degree of complication, which had the advantage that they were easily manageable, that they were promptly distinguished by our observers, though at first in non-psychological terms, and that their distinction is definitely of the kind that, in the text-books, separates memory from imagination. Even if our results are verified by other investigators, the great bulk of the chapter on the Experimental Psychology of Imagination remains to be written.

COLOR-NAMES OF ENGLISH SCHOOL-CHILDREN¹

By W. H. WINCH.

The question whether the color sense of man has undergone an appreciable evolution within historic times is one which, after half a century of discussion, still remains open.

The most considerable work on the color sense of primitive peoples known to me is that done on the Cambridge Anthropological Expedition to the Torres Straits, by Dr. W. H. R. Rivers; and that done on the Fillipinos by Dr. R. S. Woodworth.

A detailed account of the work of the former is given in the second volume of the Cambridge Reports on the expedition to Torres Straits, in part two of the volumes on Physiology and Psychology. Using Lovibond's Tintometer, Dr. Rivers found that high thresholds and defective nomenclature went together; that is to say, peoples who had no name for Blue, for example, had a much higher threshold for Blue than Europeans, whereas for Red, for which they had a well-known name, their threshold was much lower. Other converging evidences seemed to show that there was an order of color development which, after 'Light' and 'Dark,' began with Red, and was followed by Yellow, then by Green and, finally, by Blue. There was thus agreement in the main between the order of development as determined by inferences from language and the probable order as determined by "objective" methods.

The results of Professor R. S. Woodworth's work with the Fillipinos are not yet published; it would therefore be wrong to anticipate his method or conclusions; but I gather from a very able review written by him that he has not been led to conclude that the relationship above indicated between language and sensibility is at all a close one, and that, moreover, among the primitive peoples studied by him, there was no especial lack of sensibility to "Blue."

So far, then, there seems to be no knowledge *current* among anthropological experts capable of deciding the question. However, child study—the term is, perhaps, an unhappy one, and many errors have been committed in its name—may have

¹The substance of this paper was read before the British Psychological Society on Jan. 30, 1904.

been more fortunate than anthropology. For, after all, if ontogeny repeats phylogeny in any way whatever, we need not always argue from phylogeny to ontogeny; we can turn our argument about, and child study can help anthropology.

Do observations on the color-sense of children yield results parallel to those of Dr. Rivers, or of Professor Woodworth, or of neither? Is there any definite indication of development at all? I think there is, but I am very far from feeling that the question is settled.

Professor Preyer in *Die Seele des Kindes*, 1881, records experiments with a single child. The test used appeared to depend on the connection between name and color having been correctly established. There seems some doubt as to what his conclusions were. Dr. Holden and Mr. Bosse, in the *Archives of Ophthalmology* XXIX, 1900, say, in summarizing his work, "the greatest percentage of correct responses were for yellow, then for brown, red, violet, black, rose, orange, gray, green and blue in the order named." Dr. Rivers, in the *Popular Science Monthly* of May, 1901, interprets Preyer's results to mean that the child makes no distinctions of color till the end of the second year, and that 'Red' is distinguished before 'Blue.'

Preyer himself says, in the English translation of his work known as the *Infant Mind*, "I put color tests to my child for a series of years. Before he could talk, he was unable to distinguish Green and Blue as surely as Red and Yellow, and White and Black; and he confounded Green and Blue with Gray, and at a later period he confounded their names."

Binet's results differed from Preyer's; he believed Blue was the first color perceived, but, like Preyer, he experimented on one child only. Binet's experiments, related in the *Revue Philosophique* XXX, p. 583, also depended upon the connection between color and name having been established.

In both cases, however, that of Preyer and that of Binet,¹ there appears to have been a direct inference from a knowledge of the name and the order in which that name was acquired to the order in which the color was perceived.

Professor Mark Baldwin, in his *Mental Development in the Child and in the Race*, page 53, quite rightly criticised the assumption underlying the above arguments, and experimented with one of his children, beginning at the ninth month. He used a method, in which names were not employed, which he called the 'dynamogenic' method. He presented colored papers

¹I am quite sure, though I cannot find the reference, that later work by Binet makes a clear distinction between the order of perceiving and that of naming.

at different distances, and noticed to which and how much the child moved to get them. He found the order of attractiveness to be Blue, White, Red, Green. Unfortunately, however, the order of attractiveness need not be the order of perception; it is fairly certain from general psychological considerations that it is not; for we know that what is quite well known loses attractiveness, after a time, from the mere fact that it is well known.

Perhaps we may pause for a moment to express surprise, if not consternation, that our search for the order of color development has so far brought us difficulty after difficulty. We see that the problem has occupied the minds of very able men; yet little agreement, if any, can be found so far.

There is, I think, a general biologic consideration which, had it been present in the minds of those who have argued trenchantly on these facts derived from individual cases, might have saved much logomachy. Ought we to have expected exact uniformity in the development of these children? The presence of the sensory basis of color sensation—whatever that may be—is only one element or factor necessary in the perception of color. One must have exactly similar and constant environments for all observers before one can conclude that the variations found in perception are due to differences in the sensory basis. And even then why should the development of the sensory basis in every individual be quite the same even under exactly similar environments?

We must, I think, be content with something much less definite than early experimenters thought possible—the true result, perhaps, will be a kind of mean from a number of developments which are not quite the same. But I am afraid I am going ahead a little too fast; I am not aware that, up to the present, my reader will think that a case has been made out for any order of development at all; he is more likely to say, having regard to the variety of different orders so far thrust upon his notice, “A plague upon all your orders.”

But I must not leave this section of the subject without a reference to the work of Garbini, *Evoluzione del senso cromatica nei bambini*. *Arch. per l'Antropologia* XXIV. I regret to say that I have been unable to see the original paper; but I am glad to be able to express my acknowledgments of gratitude to the succinct account of it given in Dr. A. F. Chamberlain's valuable compendium, *The Child*.

Garbini employed a name method and a matching method without names, and compared the orders of the two developments—of the order in which the colors were most accurately matched and of the order in which, later in the child's life,

they were most accurately named. He experimented with about 550 children, boys and girls, between the ages of three and six; so that, with his work, we have no feeling of insecurity from the paucity of the cases. Garbini found that the order of perception was identical with the order of naming, and that this order was Red, Green, Yellow, Orange, Blue, Violet. Perhaps it may be found useful subsequently, if I give the Italian names—rosso, verde, giallo, arancio, blu, violetto.

These results are of great importance. We have (1) the fact of the development of the color sense, (2) the order in which the colors become perceptible, (3) the fact of development in color nomenclature, (4) the order in which the names of the colors become known, and (5) the exact correspondence between these two orders of development—all these important questions dealt with and settled in a way which appears, on the face of it, certainly coherent and extremely probable.

And there is very marked resemblance between the results of the work with Garbini's children, the conclusions drawn from the philological consideration of ancient language, and the results of Dr. Rivers's work both philological and experimental.

The anthropologic order, so to speak, is White—Black; Red; Yellow; Green; Blue.

Garbini's paidological order is White—Black; Red; Green; Yellow; Blue.

Garbini concluded that the child begins to see Red and Green towards the end of his second year; and that, during his third year, he begins to discern Yellow, and has the first impression of Orange, Blue and Violet; but only begins, for, from thence onwards until the sixth year and even later, the child only becomes adequately familiar with Orange, Blue and Violet.

There is no doubt that the work on children, so far, had led to the conclusion that the color sense was absent until the period just before the close of the second year, and that its growth was somewhat slow. But it was not doubted that lightness and darkness, whiteness, grayness and blackness were perceptible much earlier; and that objects were discriminated by means of their varying brightnesses before their different colors, as such, assisted the child to distinguish them in any way.

I wish now to refer again to an interesting piece of work by Dr. Holden and Mr. Bosse of New York. The paper may be found in full in the *Archives of Ophthalmology*, Vol. XXIX, No. 2, 1900. They pointed out, truly enough, that a child might reach to a colored object because of its difference in brightness from its background, even when no color could be

seen at all. One might, perhaps, go further and say that a child *might* successfully *match* a number of colored objects, putting all the reds with the reds, all the blues with the blues, and so on, simply because the reds and blues were unequal in point of luminosity.

The importance of this consideration is decidedly great, and it would certainly seem that if, by placing the colors on backgrounds of equal luminosity with themselves, we took away one means of discrimination—admittedly possessed by quite young children—and forced the child to discriminate by means of his color vision alone, we should discover that *the sensibility to color, as distinct from that to brightness, was a very much later development than had previously been supposed.*

Dr. Holden and Mr. Bosse aimed at eliminating this element of luminosity altogether; and found, by their method, that the development of color sensibility was *much earlier* than had previously been supposed. The child had now less to go by, but he discriminated better—a certainly unexpected conclusion. Is it allowable to suggest that, after all, Garbini's children were Italians, and these little ones were among the brightest and least shy inmates of the New York Nurseries?

Perhaps a word or two may be admissible as to the nature of the test. A small square of colored paper was placed on a gray of equal luminosity. "If the young child made an effort to grasp the colored square it must have perceived *that color*," say the authors. Well, I suppose we all grant that the infant did not try to pick up a piece of colored paper which he could not see; but he might see it and not grasp for it, one would think; this, indeed, is allowed by the authors, who say that the failure to reach out for the green, blue and violet might have been in part due to lack of interest in them, though they think that the perception of colors of the red end of the spectrum is acquired a little earlier than of those of shorter wave length. All these colors are seen and reached for within the first year. The method hardly allows the statement of a definite order of perception, but it seems apparent that Red, Orange, Yellow were visible quite early; Green, Blue, Violet were perceived later, though, in some cases, no evidence could be obtained that Blue was perceptible at all.

All this interesting work goes in a general way to support the anthropological view before quoted; but there are some interesting differences between these results and those of Garbini.

Dr. Holden and Mr. Bosse assert that average infants of seven or eight months old promptly react to Red, Orange and Yellow; and that, with infants of ten to twelve months of age, there was prompt reaction to Green, Blue and Violet as well.

Garbini thinks that the first chromatic perceptions occur between the sixteenth and twenty-fourth month; that these first color perceptions are Red and Green; that the child begins to differentiate Yellow during the third year, and then Orange, Blue and Violet in the order named.

The problem may be put thus: Does the child become aware of colors in the spectral order beginning from the red end, or does he find certain non-adjacent portions of the spectrum more distinct as colors at first, and perceive the intermediate and gradation colors afterwards? I put the case in this way merely as an illustration and not because I wish to insinuate that spectral order has necessarily anything to do with the question of the order of color perception in human beings. With all deference to physicists I am strongly of opinion that the construction of a psychological theory of color-development which will stand has not been much advanced by constant reference to physical problems.

Perhaps it may be suggested at this point that, so far, neither of the great color-theories have received much encouragement. It is not unfair to suppose that the so-called primary color substances would have been developed first. Now Garbini's results do seem intelligible on the Hering theory, but the Holden-Bosse results seem hard to understand on either the Young-Helmholtz or the Hering theory, which, by the way, is also the case with what we might call the anthropological order of development.

But—I am afraid there is a but—is the inference, on which Dr. Holden's results are based, sound? May we say, when the child reaches for the red, orange, yellow, green or blue paper that it must have perceived *that* color? It seems to me we should only be justified in saying so if he could distinguish these colors from each other. *Must have perceived color* would perhaps have been a fairer conclusion. This consideration may solve the awkward question as to the great difference in the ages at which American and Italian children appear to perceive the colors. Garbini's children were required to discriminate one color from another; the American children were required only to perceive color, not colors.

It is not uncommon for young children to apply with avidity the same color name to all colors; sometimes that name is Red, sometimes it is Blue, and sometimes it is Color. Now, if we are justified in believing that there is some kind of parallelism between the development of the nomenclature and the development of the sensibility—and, if we accept the results of Garbini with children and Dr. Rivers with primitive peoples, we must so believe—I should like to go a step further than has been done so far. I should like to suggest not only,

as has been said, that the order in which the names are learned is the order in which the sensibilities for the various colors are developed, but that the application of one or two names to all colors is an indication that before Red, Green, Blue and the rest are seen as colors, all these things are seen as color, and at an early stage are neither Red, Green, Blue, Yellow, nor any other sensation with which adults are acquainted, but a unitary sensation out of which certain components become more and more definite as time goes on, and more and more unlike each other; when this latter stage begins we have the perception of colors; it is this stage only which we can speak of as that of color development.

But at this point I shall be told and quite rightly, I fear, that the correlation between sensibility and language is much too doubtful to support such a superstructure of hypothesis. Arguments can be advanced on both sides; on the side of correlation it can be urged

1. That we adults ask names for new thoughts, new things and new sensations when we apprehend them, and that much more is this the case with children.

2. That the objective tests of Dr. Rivers and Garbini, coupled with name tests, exhibit certain correspondences, with Garbini a correspondence of orders of development, with Dr. Rivers a correspondence between a high threshold of perception and defective nomenclature.

An opponent could plead: firstly, that we can perceive plenty of things for which we have no names, that we do not ask names merely because we perceive things but because we are interested in them, and especially that we can see many colors for which we have no names. Mr. Hoopdriver, in Mr. H. G. Wells's *Wheels of Chance*, when for the first time he saw the flowers of the country side, marvelled that they had no names: one cannot pretend that he did not see them because he did not know their names. Secondly, that cases are on evidence, for example, the Nubians examined by Virchow, in which defective language was not accompanied by defective sensibility. Thirdly, that if these persons whose terminology for color is defective are given instruction as to the separate and several names, their color names will be found as accurate as our own, at least, in a little while, for one could hardly expect the names to be acquired without some misplacement of terms at first.

How can we overcome this difficulty as to language? It seems to me that we must find cases *in which certain color names have an equal chance of being known*, so far as instruction is concerned. Then, if some names are found to be known better than others, this may arise from two causes:

1. Because the name is easier to articulate and remember: "yellow," for example, is a hard color-name, "red" is an easy one.

2. Because the sensibility to the color is better developed; for we could hardly expect a child to learn the color name of a color which he can barely discriminate, if at all.

There is a critical consideration sometimes advanced which needs mention. It is urged that the child may discriminate all the spectral colors from one another at a very early age, but that he does not learn the names of all the colors which he can discriminate, but only of those by which he is attracted. In finding the order in which the names become known, one is therefore only finding an order of attractiveness, not an order of perception. This consideration appears to me to be a weighty one; and, were the child left to pick up his color names from information which he gets at home about colors which interest him personally, I should attach great importance to it. But the children, whose color names I ask the reader to consider in this paper, are children who learn these things at school. And the important points in this connection are that, in the infant schools which these children attend, colored objects are in use—worsted balls and beads—which are of fairly constant coloration in all schools; and that, of these things, the teachers do not, on the whole, teach the name of one color more frequently than another. If the child can easily discriminate the color, can easily articulate and remember the name, and can connect the name with the color, there is reason to suppose, on the methods of teaching employed, that he would learn the name for every color used, and not merely for those for which he felt a liking.

To me, therefore, it seemed that the conditions were especially favorable for a test with color names, and that we might, under these conditions, draw some inferences from the order of naming to the order of perception. The exercises were two in number:

1. The colored worsted balls of the Froebelian Gift 1 were arranged in the following order: Red, Orange, Yellow, Green, Blue, Violet. Each child was asked separately to name the colors. In some cases the colors were presented in the reverse order.

2. Strings of large glass beads in use in the school, the coloration and luminosity of which differed markedly from those of the worsted balls of corresponding colors, were arranged in the following order: White, Black, Red, *Amber,

*NOTE. The Amber beads were a transparent brownish yellow. It was not to be expected that young children would call them yellow.

TABLE IA
Showing the Color-Names of School "A"

Name	Sex	Age		First Exercise						Second Exercise					
		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
Initial only															
S. F.		3	3	Black	Black	—	Black	Red	Red	Black	Black	Black	Black	Red	Red
V. G.		3	3	White	—	Yellow-Red	Red	Blue	Yellow	Blue	Red	Red	Red	Red	Red
A. T.		3	3	Black	Black	Black	Red	Black	Black	Red	Black	Red	Black	Red	Red
B. C.		3	4	—	—	—	—	Blue	Blue	Blue	—	Blue	White	—	Blue
F. P.		3	5	Blue	—	Red	Red	Blue	Blue	Blue	Black	Red	Blue	Blue	Red
D. M.		3	11	Red	Red	—	—	—	—	—	Black	Red	—	—	—
C. J.		4	2	Red	Blue	Yellow	Green	Violet	Violet	White	Black	Red	Black	Yellow	Blue
M. P.		4	5	Red	Blue	Red	Dark	Red	Blue	White	Black	White	Blue	Blue	Blue
E. P.		4	6	Red	Pale Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
E. W.		4	7	Red	Dark Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
E. T.		4	7	Red	—	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
L. D.		4	10	Red	Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
N. B.		4	11	Red	Blue	Green	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue

NOTES. 1. When two color-names occur, separated by a hyphen, both names were applied in the order in which they were written.

2. The blank spaces mean that no answer was given. I should have been inclined to regard these blanks as better than errors, had it not been apparent that the older children, even when ignorant of the name which we apply, were almost invariably ready with some name which to them seemed applicable.

3. I regret that I am unable to give the sex of the children in this table.

Green, Blue. In some cases the colors were presented in the reverse order. Each child was asked separately to name the colors. No child proceeded immediately from Exercise 1 to Exercise 2. All the children tested in the same school attempted Exercise 1 first, and then, after a short interval, attempted Exercise 2.

Perhaps a few comments directly attached to the Table may make my subsequent argument clearer.

In the first group of children—whose ages are between 3 and 4—we find only a few color names in use, and these are used with what seems to us an absolute lack of discrimination. It may subsequently prove of importance if we set down in order the number of times each name is used; but, before doing so, it is worth noting that, of these six children, three—S. F., A. T., and D. M.—had *two* color names only, Red and Black. B. C. had two names only, Blue and White. F. P. had three names only, Red, Black and Blue. V. G. had four only, Red, Yellow, Blue and White.

TABLE I B

Showing the number of times each Color-Name was used in School "A"

Color-Names	Ages 3-4	Ages 4-5	All Ages
* Black	16 times	9 times	25 times
White	2 "	8 "	10 "
Red	22 "	15 "	37 "
Blue	13 "	18 "	31 "
Green	0 "	12 "	12 "
Yellow	2 "	12 "	14 "
Orange	0 "	0 "	0 "
Violet	0 "	7 "	7 "
Brown	0 "	2 "	2 "

* NOTE 1. I have included in this number the word "Dark" which was once given for "Green." The word "Black" is unusually prominent in this school. Can this be connected with the fact that the only social events of general interest in this immediate neighborhood are funerals?

NOTE 2. It will be remembered, in considering the figures of Tables marked B, that there were no Blacks and Whites among the worsted balls.

The next school at which the observations were made, School "S", was socially on a level with School 'A'; but its environment was, if anything, more colorless, and without the spice of excitement and the visual stimulation of moving things provided in the former case by proximity to a main road.

TABLE I C

Showing the percentage of Color-Names correctly used in School "A"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	4 out of 6	67	7 out of 7	100
White	0 " " 6	0	7 " " 7	100
Red	5 " " 12	42	13 " " 14	93
Blue	4 " " 12	33	12 " " 14	86
Green	0 " " 12	0	11 " " 14	79
Yellow	1 " " 6	17	5 " " 7	71
Orange	0 " " 6	0	0 " " 7	0
Violet	0 " " 6	0	6 " " 7	86

In the first group of children, ranging in age from 3 years 1 month to 3 years 10 months, we find one child who insists on responding to our questions about the names of colors, by the word 'Color,' varying it only in one case, in which she says 'Another Color.' E. S. somewhat resembles J. K.; she used the word 'Color' twice; in all other cases she used the name 'Black.' F. M., after starting well, lapsed into a color terminology of her own. 'Blue' she called 'E.' in both exercises, though they were very different Blues, and 'Green' she called 'Air,' though they were very different Greens. Three other children fall back on the word 'Color,' or 'Color-one'—F. K., V. C. and H. B. And in the second group of children—those varying from 4 years 2 months to 4 years 9 months—the oldest child is found using the word 'color' not less than three times. The theoretical interest lies in this: We have to ask ourselves how far we are justified in supposing that the word 'color,' when used by a young child, represents a concept embracing different units previously discriminated as red, blue, etc.; or whether, prior to this discrimination, 'color' does not mean for him a unitary apprehension. Then the part of the continuum which stand out earliest will, other conditions being equal, get their names first. This, at least, is a possible explanation of the very early use of the word 'color.' And it is fairly obvious that, on association principles, the word would appear much later; it would need to be generalized from its instances.

The next school at which the observations were taken was in a neighborhood decidedly superior to those of the two previous schools, and the school was very full. It was not easy, therefore, to obtain very young children—the youngest I could get was 3 years 5 months old; and I could only find four

TABLE II A
Showing the Color-Names of School "S"

Name	Sex	Age		First Exercise						Second Exercise					
		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
F. K.	B	3	1	Red	Red	Yellow	Green	Blue	Blue	White	Color	Red	Blue	Blue	
V. C.	B	3	2	White	Color-one	White	Green	Red	Red	White	White	White	White	Blue	
E. S.	G	3	5	Black	Black	Black	Color	Black	Color	Black	Black	Black	—	Black	
J. K.	G	3	9	Color	Color	Color	Color	Color	Another	Color	Color	Color	Color	Color	
F. M.	G	3	9	Red	Yellow	Orange	Air	E	Air	Air	E	Air	E	E	
H. B.	B	3	10	White	—	—	—	Blue	Blue	—	—	Yellow	—	Color	
M. McD.	G	3	10	Red	Yellow	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Blue	
E. J.	G	4	2	Red	Brown	Brown	Green	Blue	Violet	White	Color	Red	Green-Brown	Green	
W. B.	B	4	6	Red	Brown	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Blue	
R. H.	B	4	7	Red	Green	Yellow	Green	Blue	Blue	White	Black	Green	Yellow-Green	Blue	
M. M.	G	4	8	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Brown	Blue	
W. M.	B	4	9	Blue	White	Color	Green	Blue	Color	White	White	Green	Blue	Blue	

TABLE II B

Showing the number of times each Color-Name was used in School "S"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	10 times	3 times	13 times
White	10 "	7 "	17 "
Red	8 "	7 "	15 "
Blue	10 "	13 "	23 "
Green	3 "	15 "	18 "
Yellow	6 "	5 "	11 "
Orange	1 "	0 "	1 "
Violet	1 "	2 "	3 "
Brown	1 "	5 "	6 "

TABLE II C

Showing the Percentage of Color-Names Correctly used in School "S"

Color-Names	Ages 3-4		Ages 4-5	
	Times Correctly used	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	2 out of 7	29	3 out of 5	60
White	3 " " 7	43	5 " " 5	100
Red	5 " " 14	36	7 " " 10	70
Blue	6 " " 14	43	9 " " 10	90
Green	3 " " 14	21	9 " " 10	90
Yellow	2 " " 7	29	3 " " 5	60
Orange	0 " " 7	0	0 " " 5	0
Violet	1 " " 7	14	2 " " 5	40

children then present under 4 years of age: I was, however, able to get an adequate sample of children between 4 and 5.

We note at once an increase in the number of color-names. One child, K. S., on one occasion only, falls back on the word 'Color,' and only one child, D. S., has as few as two color-names. She, it will be noted, used 'Red' and 'White' only. All the other children have at least four names, though they are not, of course, always accurately applied.

The next school at which the observations were made was in a poor neighborhood with a colorless and cheerless environment, though not very far removed from the bustle and movement of a great thoroughfare.

One child, A. N., applied the name 'Blue' to every color indiscriminately; this was perhaps in her language equivalent to the word 'Color.' The latter word was not used by any child in this school; but one child used the word 'grass' for the amber and the green beads in the second exercise. The names

TABLE III A
Showing the Color-Names of School "G"

Name	Sex	Age		First Exercise					Second Exercise						
		yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
D. S.	G	3	5	Red	Red	Red	White	White	Red	Red	White	White	White	Red	White
H. H.	G	3	5	Red	—	Yellow	Green	Red	Blue	White	Black	Red	Green	Yellow	Blue
K. S.	G	3	8	Red	White	White	Green	Color	Red	White	Black	Red	Red	Green	Red
M. S.	G	3	10	Blue	Blue	White	White	Blue	White	White	Black	Red	White	Blue	Red
E. G.	B	4	2	Red	Yellow	—	Green	Blue	Blue	White	—	Red	Yellow	Green	Blue
G. G.	B	4	4	Red	Brown	Blue	Blue	Blue	Blue	White	Black	Red	Brown	Blue	Blue
E. M.	G	4	5	Red	Orange	Green(1) Yellow(2)	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
K. B.	G	4	6	Red	—	—	Red	Red	Red	White	Black	Pink	Red	Red	Red
V. G.	B	4	6	Red	Yellow	Yellow	Red	Blue	Blue	White	Blue	Red	Yellow	Red	Blue
W. B.	B	4	9	Red	Orange	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
A. H.	G	4	10	Red	Yellow	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue

TABLE III B

Showing the number of times each Color-Name was used in School "G"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	3 times	5 times	8 times
White	14 "	7 "	21 "
Red	17 "	21 "	38 "
Blue	6 "	22 "	28 "
Green	4 "	9 "	13 "
Yellow	2 "	12 "	14 "
Orange	0 "	2 "	2 "
Violet	0 "	0 "	0 "
Brown	0 "	2 "	2 "

TABLE III C

Showing the Percentage of Color-Names correctly used in School "G"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	3 out of 4	75	5 out of 7	71
White	3 " " 4	75	7 " " 7	100
Red	7 " " 8	87	13 " " 14	93
Blue	2 " " 8	25	12 " " 14	86
Green	3 " " 8	37	8 " " 14	57
Yellow	1 " " 4	25	4 " " 7	57
Orange	0 " " 4	0	2 " " 7	29
Violet	0 " " 4	0	0 " " 7	0

are generally more developed among these children, though one boy, G. C., has three names only—Red, White Black.

The school at which similar observations were next taken was situated in a better neighborhood, and stood beside a park. The word Green is, undoubtedly, a more prominent feature than usual in the vocabulary of these children, though the average age of both groups, namely that consisting of children from 3 to 4 years of age, and that of children from 4 to 5, is low.

At the commencement of the table given above, we are fortunate in having five very young children, one of whom, C. M., has one color-name only—'Red,' which she applies to *all* colors; it is her word for color. G. B. has two color-names only, 'Blue' and 'Dark.' T. F. has three only, 'Green,' 'Black,' 'White.' S. L. and W. H. have three names only, 'Red,' 'Green,' 'White.' E. H., who is several months older, has three names only, 'Red,' 'Black' and 'Green.' The great

TABLE IV A
Showing the Color-Names of School "R"

Name	Age		First Exercise					Second Exercise						
	Sex	yrs. ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
Initials only														
A. N.	G	3	1	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue	Blue
G. C.	G	3	3	Red	White	Red	White	Red	White	Black	Red	White	Green	White
G. C.	B	3	5	Red	White	Red	White	White	White	Black	White	Red	White	White
B. B.	B	3	8	Yellow	—	—	Blue	Blue	White	Black	Red	Grass	Grass	Blue
M. H.	G	3	8	Blue	White	Blue	White	Blue	White	Black	Black	White	Black	Blue
J. C.	G	3	8	Red	Yellow	Green	Green	Blue	White	Black	Black	Green	Green	Blue
A. P.	G	3	11	Black	White	Green	Green	White	Green	Black	—	—	Green	Blue
E. N.	G	4	3	Red	—	Green	Green	Blue	White	Black	Red	—	Green	Blue
C. B.	G	4	3	Red	White	Blue	Blue	Red	White	Blue	Red	—	Gay	—
B. C.	B	4	5	Red	Yellow	Green	Green	Blue	White	Black	Red	Brown	Green	Blue
F. B.	G	4	7	Red	Yellow	Green	Green	Blue	White	Black	Red	—	Green	Blue
L. W.	G	4	11	Red	Yellow	Green	Green	Blue	White	Black	Red	Brown	Green	Blue
M. C.	G	4	11	Red	Brown	Blue	Blue	Blue	White	Black	Red	Yellow	Blue	Blue

TABLE IV B

Showing the number of Times each Color-Name was used in School "R"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	10 times	5 times	15 times
White	21 "	9 "	30 "
Red	10 "	13 "	23 "
Blue	23 "	19 "	42 "
Green	7 "	9 "	16 "
Yellow	4 "	7 "	11 "
Orange	0 "	0 "	0 "
Violet	1 "	0 "	1 "
Brown	0 "	3 "	3 "

TABLE IV C

Showing the Percentage of Color-Names correctly used in School "R"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	6 out of 7	86	5 out of 6	83
White	5 " " 7	71	6 " " 6	100
Red	5 " " 14	36	12 " " 12	100
Blue	8 " " 14	57	10 " " 12	83
Green	5 " " 14	36	8 " " 12	67
Yellow	2 " " 7	29	3 " " 6	50
Orange	0 " " 7	0	0 " " 6	0
Violet	1 " " 7	14	0 " " 6	0

feature of interest in this table is the very early appearance of 'Green.' It is noteworthy that the elder group were much better acquainted than usual with names like 'Violet,' 'Orange,' 'Brown,' so that the earlier appearance of 'Green' may not be due to environment, as I suggested in a previous similar case, but to the slightly superior social class of the children.

The school in which the next observations were made is situated in a somewhat poor district; but one in which the small house and long garden, once characteristic of London's poorer suburbs, have not been entirely swept away by modern improvements!

The two youngest children, C. N. and W. N., were very deficient in color names, as was L. P., who was six months older—an important difference at this age, so far as the acquisition of language is concerned. L. P. is another case to be added to those which we have already found, in which the

TABLE V A
Showing the Color-Names of School "P"

Name	Age		First Exercise					Second Exercise						
	Sex	yrs. ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
G. B.	B	3	Blue	Blue	Blue	Dark	Blue	Dark	Blue	Blue	Blue	Dark	Blue	Blue
T. F.	G	3	White	—	—	Green	White	White	White	Black	White	White	Green	White
S. L.	B	3	Red	White	White	Green	—	Red	—	Red	—	Red	White	White
W. H.	B	3	Green	Red	Green	Red	Green	Green	White	Red	White	White	Green	Green
C. M.	G	3	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red
E. H.	B	3	Red	Green	Green	Red	Red	Red	—	Black	Red	—	Red	Red
T. B.	B	3	Red	Red	Like butter	Green	Blue	Blue	White	Black	—	Green	Green	Blue
C. M.	G	3	Red	Yellow	—	Green	Blue	Blue	—	Black	Red	—	—	Blue
L. B.	G	4	Red	Orange	White	Green	Blue	Blue	White	Black	Red	Brown	Green	Blue
H. O.	B	3	Red	Yellow	—	Green	Blue	Violet	White	Black	Red	—	Green	Blue
A. C.	B	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
N. H.	G	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
C. M.	B	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Brown	Green	Blue
A. H.	B	4	Red	—	White	Green	Blue	Blue	White	Black	Red	Green	Green	Blue

TABLE V B

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	4 times	6 times	10 times
White	16 "	8 "	24 "
Red	30 "	12 "	42 "
Blue	15 "	14 "	29 "
Green	14 "	13 "	27 "
Yellow	1 "	4 "	5 "
Orange	0 "	4 "	4 "
Violet	0 "	4 "	4 "
Brown	0 "	4 "	4 "

TABLE V C

Showing the Percentage of Color-Names correctly used in School "P"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	4 out of 8	50	6 out of 6	100
White	3 " " 8	37	6 " " 6	100
Red	8 " " 16	50	12 " " 12	100
Blue	6 " " 16	37	12 " " 12	100
Green	6 " " 16	37	12 " " 12	100
Yellow	0 " " 8	0	3 " " 6	50
Orange	0 " " 8	0	4 " " 6	67
Violet	0 " " 8	0	4 " " 6	67

word 'Red' is the generic color-name; C. N. has the word 'White' as well. W. N., though tried in a variety of ways, beyond those specified in the exercises, was not found to be the possessor of any color-name at all; he spoke quite freely but used no color words which his teachers or I could recognize or even see any constancy in as applied to colors.

The most striking feature in this table is the complete absence of the word 'Blue' in the younger group, and its infrequency as compared with 'Green' in both groups. These facts should be noted since they are, I think, quite exceptional as far as these tables are concerned.

The next observations presented were made at a school situated in a decidedly better neighborhood than the two preceding ones. It stands facing a large open space with many trees, much grass, and some flowers.

From one child, T. W., we failed altogether to elicit any words which appeared to have reference to color; but generally speaking there was decided superiority in color vocabulary in this school. 'Green,' again, as in the previous case in

TABLE VI A
Showing the Color-Names of School "C"

Name		Age		First Exercise					Second Exercise						
Initial only	Sex	yrs.	ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
C. N.	B	3	1	White	White	White	White	—	—	Red	Red	Red	—	White	White
W. N.	B	3	1	—	—	—	—	—	—	—	—	—	—	—	—
L. P.	G	3	7	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red	Red
B. H.	G	3	8	Red	White	—	Green	Red	Red	Red	Black	Red	White	Green	Red
L. S.	G	4	0	Red	—	Blue	Green	White	—	White	Red	Red	—	Green	—
J. P.	G	4	8	Red	Brown	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
T. A.	B	4	8	Red	Violet	—	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
N. S.	G	4	9	Red	Violet	Yellow	Green	Blue	Violet	White	Black	Red	Violet	Blue	Violet
W. N.	B	4	10	Red	—	Yellow	Green	Violet	Violet	White	Black	Red	Yellow	Green	Blue

TABLE VI B

Showing the number of times each Color-Name was used in School "C"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	1 time	4 times	5 times
White	10 times	4 "	14 "
Red	24 "	8 "	32 "
Blue	1 "	7 "	8 "
Green	4 "	7 "	11 "
Yellow	0 "	6 "	6 "
Orange	0 "	0 "	0 "
Violet	0 "	9 "	9 "
Brown	0 "	1 "	1 "

TABLE VI C

Showing the Percentage of Color-Names correctly used in School "C"

Color-Names	Ages 3-4		Ages 4-5	
	Times used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	1 out of 5	20	4 out of 4	100
White	1 " " 5	20	4 " " 4	100
Red	7 " " 10	70	8 " " 8	100
Blue	0 " " 10	0	6 " " 8	75
Green	4 " " 10	40	7 " " 8	87
Yellow	0 " " 5	0	3 " " 4	75
Orange	0 " " 5	0	0 " " 4	0
Violet	0 " " 5	0	4 " " 4	100

which the school fronted a park, was better known than usual. Only one other child, H. T., had less than four color-names, and he had 'White,' 'Black' and 'Red.'

It is impossible to read the foregoing tables without coming to the conclusion that, both in the younger and older groups, there is much agreement as to the order of the accuracy with which the color-names are applied. The names Black, White and Red are used with approximately equal accuracy by both these groups of children who, as I have said, have been taught equally to name all these colors. There can, too, I think, be little doubt that Blue occupies the next place in order of accuracy, that Green follows Blue, and is, in its turn, followed by Yellow, whilst Violet and Orange are quite at the bottom of the list.

It must not be supposed, however, that we are quite sure that every *one* of these children had been taught exactly equally every *one* of these color-names. The three-year-old group in school

TABLE VII A
Showing the Color-Names of School "D"

Name	Age		First Exercise					Second Exercise						
	Sex	yrs. ms.	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Amber	Green	Blue
T. S.	B	3	White	—	White	Green	White	White	White	Black	Green	White	Red	Green
T. W.	B	3	—	—	—	—	—	—	—	—	—	—	—	—
C. H.	B	3	Green	Yellow	—	Green	Blue	Blue	White	Black	Red	Green	—	—
H. T.	B	3	White	White	Red	Red	Red	Red	White	Black	White	Red	Red	Red
E. H.	G	3	Red	Red	—	Green	—	—	White	Black	Red	—	—	Blue
D. P.	G	3	Red	—	Yellow	Green	Blue	Violet	White	Black	Red	—	—	Blue
L. P.*†	B	3	—	Red	—	Red	Pink	Red	Blue	Dirty- Blue	—	Blue	Beads of White	Blue
L. S.*	B	3	Red	Red	Yellow- Red	Blue	Blue	Blue	Wash	Blown	Red	Red	Green	Blue
E. H.*	B	3	Green	Green	White	Green	White	Green	White	Black	Green	Green	Green	Blue
A. D.*	B	3	Red	Blue	White	Red	Blue	Color	White	Black	Blue	Color	Blue	Blue
T. E.	B	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
M. F.	G	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
T. W.	B	4	Red	Orange	Yellow	Green	Blue	Violet	White	Black	Red	Yellow	Green	Blue
M. B.	G	4	Red	Orange	Yellow	Green	Blue	Blue	White	Black	Red	Yellow	Green	Blue
R. D.	B	4	Red	Yellow	Yellow	—	Green	Violet	White	Black	Red	Yellow	Green	Blue
W. W.	G	4	Red	Orange	Green	Green	Yellow	Orange	White	Red	Red	Yellow	Green	Blue

* In these four cases the balls and beads were presented in the reverse of the usual order.

† L. P. volunteered the information that the blue beads were like the red beads.

TABLE VII B

Showing the number of times each Color-Name was used in School "D"

Color-Names	Ages 3-4	Ages 4-5	All Ages
Black	7 times	5 times	12 times
White	19 "	6 "	25 "
Red	25 "	13 "	38 "
Blue	19 "	11 "	30 "
Green	17 "	13 "	30 "
Yellow	3 "	13 "	16 "
Orange	0 "	6 "	6 "
Violet	1 "	4 "	5 "
Brown	1 "	0 "	1 "

TABLE VII C

Showing the Percentage of Color-Names Correctly used in School "D"

Color-Names	Ages 3-4		Ages 4-5	
	Names used Correctly	Percentage of Correct Ans.	Times used Correctly	Percentage of Correct Ans.
Black	7 out of 10	70	5 out of 6	83
White	7 " " 10	70	6 " " 6	100
Red	8 " " 20	40	12 " " 12	100
Blue	10 " " 20	50	10 " " 12	83
Green	8 " " 20	40	11 " " 12	92
Yellow	2 " " 10	20	5 " " 6	83
Orange	0 " " 10	0	5 " " 6	83
Violet	1 " " 10	10	4 " " 6	67

TABLE VIII

Summary, Showing the Percentage of Color-Names Correctly used in all Schools.

Color-Names	Ages 3-4								Ages 4-5							
	School	School	School	School	School	School	School	Average	School	School	School	School	School	School	School	Average
	A	S	G	R	P	C	D		A	S	G	R	P	C	D	
Black	67	29	75	86	50	20	70	57	100	60	71	83	100	100	83	85
White	0	43	75	71	37	20	70	45	100	100	100	100	100	100	100	100
Red	42	36	87	36	50	70	40	52	93	70	93	100	100	100	100	94
Blue	33	43	25	57	37	0	50	35	86	90	86	83	100	75	83	86
Green	0	21	37	36	37	40	40	30	79	90	57	67	100	87	92	82
Yellow	17	29	25	29	0	0	20	17	71	60	57	50	50	75	83	64
Orange	0	0	0	0	0	0	0	0	0	0	29	0	67	0	83	26
Violet	0	14	0	14	0	0	10	5	86	40	0	0	67	100	67	51

'C,' for example, had done no previous work with the beads; hence, probably, their low mark for Black and White, for there were *no* blacks or whites among the worsted balls; and in the case of Blue, half at least of the chances of correct naming were absent also. I am quite unable to explain why, in the three-year-old group in School 'A,' the names White and Green should not have been accurately applied even once, for it is quite certain that the green ball of the Froebelian Gifts had been used. But, despite some anomalies, we may lay claim, on the whole, to have made out the order of accuracy with which the names are applied; and when we remember that there are neither black nor white balls in the Froebelian Gift 1, with which most of the early teaching of color is done, we shall probably accept the order of the correct application of color-names as

Black — White : Red : Blue : Green : Yellow : Violet : Orange.

But is it supposed that we are able from this order to infer a similar order in the development of the child's sensibilities to various colors? Let us deal for a moment with what is *not* asserted. It is *not* asserted that because a child cannot apply the word White correctly that he has no sensibility to White. It is *not* asserted that because a child says Red when looking at Green that we therefrom may infer that the child sees no difference whatever between Red and Green. I have no doubt at all, both from my own observations and from the work of others, that the discrimination of colors is a much earlier thing than the correct naming of them. The naming follows the sensibility and it follows *longo intervallo*, let us agree on that. But is the order in which the names are acquired, *provided that the child has equal opportunities of learning them and connecting them with the appropriate colors*, an indication of the order in which the sensibility to the different colors was acquired or became developed?

Garbini maintained not only that it was, but that he had shown, by separate tests on sensibility and vocabulary, that, for Italian children, the two orders coincided. The color-names became known in the order :

Rosso, Verde, Giallo, Arancio, Blu, Violetto, and this is exactly the order, according to Garbini, in which, with younger children, the sensibilities to these various colors were developed.

If Garbini's order coincided with my own, I should doubtless feel additional confidence in our common opinion that the *name-order* may indicate a *sensation-order*. I do not *know* that Garbini's children had had, generally speaking, equal opportunities of becoming acquainted with all the names. If they

had, as is quite possible, we must conclude either, 1. That the Italian order of sensational development is different from our own; or, 2. That the difficulties of learning the Italian names do not correspond to the difficulties in learning our English color-names.

If the former proposition be true, it seems that we must each rely wholly on national researches, for it is hard to believe in an Italian order without believing also in a German order, and in a French order, and, perhaps, in an American order. Personally, I should find this variety of orders hard to accept; but my reader must decide for himself.

If the second proposition be true, we *might* get a satisfactory explanation of the difference found; but it hardly seems that Italians would find *giallo*, pronounced djah'loh, so much easier than our children find *yellow*, that with them yellow would become known third and with us sixth. Blue is not a particularly easy word for a young English child, yet 'blue' with us comes second, and with Italian children the corresponding word 'blu' is learnt fifth.¹

Let us turn for a moment to corresponding work in German. I do not propose to give any detailed account of individual researches; but from the admirable and comprehensive summary given by Professor Meumann in his *Experimental Pedagogy*,² we learn that the order in which color-names become known to German children is as follows:

Weiss—Schwarz: Rot: Blau; Grün: Gelb: Violett-Orange.

Now this is exactly the order in which the names become known to London children: We are more inclined to doubt a specific Italian order after this, unless we can show that the difficulties in the German vocabulary coincide with the difficulties in our vocabulary, whereas the Italian articulatory difficulties do not. Obviously, however, we cannot show this. Our word Yellow is a hard one for our children, Gelb is easy for German children, yet Yellow is learnt sixth in both countries. The difficult German word is Grün. Any one who has ever heard German teachers laboriously, and somewhat unsuccessfully, teaching the pronunciation of words containing the modified 'u' will have a very present sense of the difficulty of the word Grün. On the other hand 'green' is for English children a word of moderate difficulty only—yet green is learnt fifth in both countries. Rot and Red are of similar difficulty: Blau and Blue, Weiss and White, Violett and Violet, Orange and Orange, are of similar difficulty; and it does not surprise us to

¹This argument fails if the children are taught the word 'azzurro' for 'blue.'

²Einführung in die Experimentelle Pädagogik, Vol. 1, page 108.

see them occupy exactly similar positions in both countries. But in Schwarz, Grün and Gelb we have words by no means corresponding in difficulty of pronunciation to Black, Green and Yellow, and yet *the same color is named in the same order of accuracy in Germany and England.*

It certainly does appear that there is in operation here a common factor which is strong enough to overcome the difference in order which may have been produced by the differences in the difficulty of the names.

Perhaps, at this juncture, it might be well to refer to a criticism on the method which was urged by Professor Sully during a discussion on the Color Sense at a meeting of the Anthropological Society. The children, he urged, would not have had equal chances of learning the color-names, because some of them would be taught the names at home. I fear, however, that these Elementary School children would not, in scarcely any case, receive instruction in color naming at home. They might have, and probably had, picked up something from hearing the talk of the elder members of the family. But the vital point is that the color preferences¹ of their elder brothers and sisters and fathers and mothers, and hence the colors they would hear most about are not such as would lead to knowledge in the order White—Black: Red: Blue: Green: Yellow. I do not wish to exclude the home talk as an altogether irrelevant factor, but I incline *à priori* to the view that it is of little weight in this case, and from the results it is apparently not operative.

During the course of these observations I learnt much, both positively and negatively, from the experienced teachers who were kind enough to assist me. When they discovered that I was best pleased with the child who was *trying* to name rightly, but who could not yet succeed, and that the boy or girl who rattled them all off glibly did not arouse my interest, they began to look at the matter in an objective spirit (it is fair to the teachers to say that I was then an Inspector of Schools) and took much interest in the experiments. Several of them, who, during the first exercise, heard the children confidently applying the wrong names, said to me: "Oh, they don't know, and are just saying anything." I might have answered that they were not exactly saying *anything*, they were, in almost every case, confining themselves to color-names. That limitation, by itself, counted for something. And when we came to the second exercise and found the child consistent in error, as he often was, the teachers gave

¹ Winch: Color Preferences of School Children. *British Journal of Psychology*, Dec., 1909.

up that explanation, especially as they saw that, both in luminosity and color, the beads of the second exercise differed much from the worsted balls of the first exercise.

Yet I am by no means indisposed to admit that some naming occurred which seemed guided by little else than an indefinite association between all color sensations and all known color names. Professor Preyer, in the *Infant Mind*, page 13, in my judgment, overworks this factor. He says, referring to the way in which children call colors by their wrong names:

"They know the words which name colors, and they have the sensations of color all right, but they do not know which words and colors belong together. They do not understand their own sensations of color." Let us hope the last sentence is a mistranslation, for it is irrelevant to the issue, if not misleading—do any of us *understand* our own sensations of color? Professor Preyer's explanation, which resembles, in another form, the teachers' explanation—that the children were just saying anything—admits of a simple verification. I urge that it is not wholly satisfactory.

If there are no connections between the names and the particular sensations, but only between color and color-names, of which *any of the latter may be given to any of the former until the right one is learnt*, then the errors made should be scattered over the colors equally. If 'Red' is not known rightly in connection with its name, it will be called Green, Blue, Yellow an equal number of times, or, at least, approximately so, provided the child can say these names and knows as, *ex-hypothesi*, he does, that they *are* color names. Let us see, by reference to our tables, how the errors are distributed. How many times, for example, is Red called Green, how many times is it called Yellow, and so on?

Red was named	White	13	times
"	"	"	Black 8 "
"	"	"	Blue 9 "
"	"	"	Green 7 "
"	"	"	Yellow 2 "
Blue was named	White	15	"
"	"	"	Black 3 "
"	"	"	Red 23 "
"	"	"	Green 5 "
"	"	"	Yellow 1 "
Green was named	White	9	"
"	"	"	Black 3 "
"	"	"	Red 26 "
"	"	"	Blue 16 "
"	"	"	Yellow 3 "

Yellow	was	named	White	28	“
“	“	“	Black	4	“
“	“	“	Red	16	“
“	“	“	Blue	8	“
“	“	“	Green	10	“

As it was decidedly unfair to count the answers to the questions as to the color of the transparent brownish-yellow or amber beads as involving knowledge or ignorance of yellow, the number of answers given as to the color of the yellow worsted ball is doubled, so as to compare with those given for Red, Blue, and Green.

If any one of my readers cares to work out the distribution of these errors among the children of various ages and various schools, he will find that the totals are fairly representative of all the children, and are not mere haphazard totals. That being the case, we may safely draw the conclusion that some general tendencies are at work; there is not a chance distribution of error over all the color names. There appears to be no tendency to use the term Yellow; this would be intelligible on the hypothesis that the word is very much harder to learn and to say than the other color names. But this hypothesis would not explain why the color is very frequently called 'White,' less frequently 'Red,' sometimes 'Blue' and 'Green,' and scarcely ever 'Black.' It staggers us a little to see that it is called 'Blue' as many times as it is, *though only about as many as would be found on a chance distribution.* Especially are we surprised, since Blue is only once called 'Yellow.'

The green ball was a dark green; the green beads were a light green; we might, therefore, have been disposed to think that the mistakes, 'White' and 'Black,' would be equal in number; but there is, except for 'Red,' no considerable number of errors in which the name 'Black' is used. Green seems to be called in error 'Red,' 'Blue,' and White. It seems good to us adults to call green 'Blue,' especially the dark green; nor does it seem unintelligible to us to call a light green 'White'; but to call green 'Red,' as was done no less than 26 times, seems to us sheer perversion. Would it be quite unreasonable to suppose that there are many cases in which, for children, the term 'Red' embraces all color sensation from the red end of the spectrum up to, and including, the green?

The errors for Blue present some expected results and some difficulties. There is no tendency to confuse with Black, as is decidedly the case with primitive peoples. There are no confusions with Yellow; we should not expect any. The term 'White,' however, is not more frequently applied to the light beads than to the dark blue worsted balls. The outstanding

point is the confusion with Red. Perhaps it is explained thus: 'Red' is the first-color name, Blue is the second; does Red then stand at first rather for color than for Red as we know it? If early color sensation is a much more unitary thing than with us adults, such a condition of nomenclature is not impossible. Adults would expect many confusions of Blue with Green, but there are only five, that is, less than a merely chance number if the errors were scattered equally over all the well-known color names.

Again in the errors for Red there are some difficulties. It is hard to see why the dark red worsted ball should be called 'White,' as it often was, and the light red beads called 'Black,' as they sometimes were. One might fall back on the theory of chance distribution for the Black, Blue and Green; but such a theory would hardly apply to the errors of White and Yellow. 'Blue' is the second color-name acquired and we should perhaps expect that it may, in some cases, even precede 'Red'; for there are certainly considerable individual differences. There are a few confusions with Green, but nothing like the number we should expect if the sensations Green and Red were as closely alike as might appear from the large number of times which Green was called 'Red.' Green we might say is called 'Red,' but Red is not called 'Green.'

Green, of course, is the third color-name in order of acquisition, and hence we might simply regard the errors as the tendency of a well-known name to spread beyond its primary application. The errors could very largely be explained in that way were it not for the small number of times which the word 'Black' is used. Only with the Red does the error 'Black' approximate to its chance number; in all other cases, though a well-known and correctly-applied name, it is not a name given to other colors, whereas the very reverse is the case with 'White.' There are evidently factors operating here which give a distribution of error much unlike what would be found on the solution that the children *just say anything*, if they do not know the right name. They *appear* to be sensationally guided; the names *appear* to be attached or in process of attachment. I do not wish to deny that the child's manner sometimes leads us to believe that he will attach any color name to any color percept, nor that there is, in these cases, a chance distribution of error. But, that another factor is working also, the analysis is sufficient proof.

What is that factor? I suggest that we only need to apply a certain principle a little lower down in the evolutionary scale which we already apply higher up. A person calls Violet 'Blue' or Orange 'Yellow'; or at night he calls Green 'Blue.' "That's very natural," we say; meaning that these colors

look to us so similar that we are not surprised that people sometimes call them by the same name. We should not call Red by the names 'Blue' or 'Green'; nor should we call Blue and Green by the name 'Red.' But children do. Shall we suppose, therefore, that these colors look so much more alike to them that they see no incongruity in using the same name; just as most of us who are not botanists are accustomed to group together all the lovely varieties of the grasses under one undistinguishing term "Grass"?

Would it be hopelessly rash to suggest that color sensation is at first unitary, that it differentiates in the order Red, Blue, Green, Yellow, Violet, Orange? As each part, if this word is applicable, of the *continuum* becomes distinct, it tends as a growing power always does, sensational or conceptional, to be seen as covering too much, and a period of contraction follows: we know what it is and more than it is, and then we know what it is not. By-and-by, if these color 'units' receive names in the order in which they have themselves been differentiated from the color *continuum*, we can, by testing for the names at a later period, find out the order of sensational differentiation at an earlier period.

Obviously, why not try a method, as Garbini did, for testing the color sensations of young children as well as for testing the application of their color names? There will be difficulties here in getting sufficient numbers of unselected children; I mean accidentally selected, of course; using the word accident in its logical sense. On this work I have been employed, on and off, for a year or two, and hope to present the results some day. My purpose, however, in this paper, is to show what may perhaps be inferred from the color names of children who have had approximately equal sensational and verbal environments.

PRACTICE IN THE CASE OF ADDITION.

By EDWARD L. THORNDIKE, Teachers College, Columbia University.

The experiment reported here was designed to secure information concerning only the amount and rate of improvement and the value of the practice experiment as a method for school work. The practice was not continued long enough nor taken under uniform enough conditions to justify inferences concerning changes in the rate of improvement; and I shall make no attempt to analyze out the factors producing the improvement.

The experiment consisted in adding daily for seven days forty-eight columns each of ten numbers (no 1's or 0's being included). Seven printed blanks had been arranged of equal difficulty.¹ The forty-eight sums were written. The time required was recorded in seconds. The subjects were nineteen university students—eight men and eleven women.

The time taken and the number of examples wrong for each set for each of the nineteen subjects are recorded in Table I. Table II repeats Table I with an addition of one per cent. of the time for forty-eight examples for each example wrong. That is, I estimate that half the time for one example is a just allowance to balance its inaccuracy. This system of allowance is, of course, arbitrary, but it will not prejudice any of the conclusions which I shall draw. They would be the same by any reasonable allowance. Table III summarizes conveniently the facts as to the amount and rate of improvement, and its relation to initial ability.

Taking the whole group together, improvement in speed and in accuracy are about equal, the median reduction in time regardless of errors being 31 per cent. and the median reduction in errors regardless of time being 29 per cent. By the scores with allowance for errors the median improvement in general efficiency in addition is 33 per cent. The average improvement is 29 per cent. This is for less than one hour of practice (about fifty-three minutes).

The individuals vary from cases making no improvement (F and G) to a case of nearly fifty per cent. improvement (K).

That the practice represented by only 2,592 additions made by an educated adult whose addition associations have been long established and often used should produce an improvement of three-tenths, bears witness to the continued plasticity or educability of the synapses involved. It also supports the contention that the degree of efficiency shown by persons in any intellectual function is a result chiefly of specific training in it or the elements of it and only slightly of the transfer to it of the effects of training other functions. If the general training of from fifteen to twenty-five years of a scholarly life were responsible for a large fraction of one's efficiency in "quickness of association" or "accuracy in response," one would not by so little specific training be able to improve so much.

The amount of improvement in this experiment may also add to our confidence that the method of the practice experiment wherein

¹ The improvement is measured from the average of series 1 to the average of series 7, that is, over 6 X 48 examples, each involving nine additions.

one works at one's limit and competes with one's own past record may well be made a regular feature in many school drills. Even if the same length of time produced in children a percentile improvement only half as great as here, the gain would still probably be far greater than the gain by any of the customary forms of drill.

TABLE I
Gives Scores in the Seven Successive Practice Periods

Individual	Sex	Day of beginning	Intervals between tests Hours	First		Second		Third		Fourth		Fifth		Sixth		Seventh	
				Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong	Time	Examples wrong
A	m	M	24	550	3	530	5	420	3	420	4	430	4	402	2	382	4
B	m	T	24	620	2	420	4	325	3	420	7	360	9	360	13	345	5
C	m			560	4	570	4	545	4	515	4	480	1	455	4	470	2
D	m	W	24 except 48 between 4-5	482	18	470	11	420	6	360	9	420	8	300	9	302	8
E	m	W	24 except 48 between 4-5	1020	3	810	3	770	4	680	5	735	6	650	7	605	1
F	m	W	24 except 48 between 4-5	800	3	710	2	715	2	700	3	695	1	655	4	785	3
G	m		not reported	600	0	560	0	510	0	465	0	465	4	468	4	455	0
H	m	T	24 except 72 between 4-5	456	5	390	4	325	3	329	3	368	7	320	3	310	2
I	f	W	24 except 72 between 3-4	420	2	360	0	450	0	450	1	450	1	480	4	470	4
J	f	W	24 except 48 between 4-5	641	3	630	5	608	3	640	5	550	1	545	4	475	0
K	f	W	24 except 48 between 4-5	460	0	375	1	362	0	307	0	297	3	243	2	240	1
L	f	T	24 except 48 between 4-5	560	8	520	3	540	3	440	0	500	0	390	3	385	5
M	f	T	24 except 48 between 5-6	590	4	420	3	360	0	350	1	345	0	330	0	360	0
N	f	T	24 except 48 between 5-6	750	8	740	2	605	1	600	3	650	5	550	4	500	6
O	f	W	24 except 48 between 2-3	420	5	680	1	470	1	495	0	435	3	434	3	410	0
P	f		not reported	870	14	840	11	795	13	630	16	610	7	600	5	530	10
Q	f	W	24 except 48 between 4-5	840	4	750	1	690	1	675	0	700	1	645	0	630	0
R	f	W	24 except 48 between 4-5	690	2	625	3	580	1	510	3	560	3	555	3	470	3
S	f		not reported	866	2	680	1	725	1	595	3	590	1	580	2	520	2

TABLE II

Scores Reduced to Single Variables by Allowance for Examples wrong

Indi- viduals	Sex	First	Second	Third	Fourth	Fifth	Sixth	Seventh
A	m	565	555	432	436	446	410	398
B	m	632	436	345	448	390	405	362
C	m	582	587	567	536	485	473	479
D	m	570	520	445	392	454	327	326
E	m	1050	834	800	714	780	695	611
F	m	824	724	729	721	702	681	808
G	m	600	560	510	465	483	486	455
H	m	479	406	335	339	393	330	316
I	f	428	360	450	435	455	499	489
J	f	660	662	626	672	555	550	475
K	f	460	379	362	307	306	248	240
L	f	606	535	556	453	500	402	404
M	f	614	432	360	354	345	330	360
N	f	810	755	611	618	670	572	530
O	f	441	687	475	495	448	447	410
P	f	992	932	897	730	653	630	583
Q	f	874	758	697	675	707	645	630
R	f	707	644	586	525	377	571	484
S	f	883	687	732	613	596	592	530

The relation of the amount of improvement to initial ability in any practice experiment is of great interest because it gives evidence bearing upon the fundamental problem of the relative shares of original nature and environment in determining the achievements of men.

It has been shown that in the case of educated adults the relative (that is, percentile) differences amongst educated adults in the ability to multiply mentally a three-place number by a three-place number are left unreduced by submitting all the individuals to equal practice.

The differences amongst individuals in the ability to add seem to be due in larger measure to differences in environmental influence. For equal practice does here reduce a little the relative or percentile differences within our group. This will be seen by comparing the relative variability of the group in the seventh practice period with that in the first, or by calculating the co-efficient of correlation between initial ability and percentile improvement. The proportions for highest and lowest individuals, next to highest and next to lowest are:

	1st 19th	2nd 18th	3rd 17th	4th 16th	5th 15th
In first practice period	2.45	2.25	1.92	1.82	1.46
In second practice period	3.36	1.99	1.87	1.62	1.46

The correlation between initial ability and percentile improvement is negative, roughly— $\frac{1}{4}$.

There is, of course, no essential conflict between this result for addition and the opposite result for mental multiplication with two

three-place numbers. The same theoretical view which would expect mental span and ability to manage very complex relationships in a given field to be increased by practice in close dependence upon original capacity, would expect particular associative habits such as thinking of thirteen upon seeing 4, 7 and 2 in a column, to be increased by practice in less close dependence upon original capacity.

TABLE III

The Amount of Improvement in Relation to Initial Ability

Individual	Sex	Initial Ability	Gross Improvement	Percentile Improvement
A	m	565	167	.30
B	m	632	270	.43
C	m	582	103	.18
D	m	570	244	.43
E	m	1050	439	.42
F	m	824	16	.02
G	m	600	145	.24
H	m	479	163	.34
I	f	428	61 (loss)	.14 (loss)
J	f	660	185	.28
K	f	460	220	.48
L	f	606	202	.33
M	f	614	254	.41
N	f	810	280	.35
O	f	441	31	.07
P	f	992	409	.41
Q	f	874	244	.28
R	f	707	223	.31
S	f	883	353	.40
Medians,		614		33
Averages,		674		29

The improvements recorded are of the seventh set of 48 ten-figure examples over the first such set. They represent approximately the practice effect of 2,192 additions, or of from 30 to 75 minutes work.

THE RELATION BETWEEN MEMORY FOR WORDS AND MEMORY FOR NUMBERS, AND THE RELATION BETWEEN MEMORY OVER SHORT AND MEM- ORY OVER LONG INTERVALS.

By EDWARD L. THORNDIKE, Teachers College, Columbia University.

Measurements of mental relationships are so important and so scanty that I venture to report certain ones in the case of memory, although they are by no means satisfactory with respect to method. They will not, however, be misleading to any one who bears in mind their limitations.

The measurements are of the relations in educated adults:— (1) between (a) the ability to remember a list of twelve words from a single hearing, at a rate of approximately one per second, long enough to write them immediately at the close of the reading; and (b) the same ability in the case of a list of five three-place numbers. There were five lists of (a) and five lists of (b). (2) Between: (a) and (c) the ability to remember the sixty words given in the five tests of (a) twenty-four hours later.

No requirements were made as to the order except, of course, that the order of the digits within each three-place number must be correct. The basis of the memory of (c) was not only the single hearing, and the experience of writing down such words as the individual remembered, but also the experience of scoring one's results from a complete list given to the individual for that purpose.

(1) and (2) do not, that is, measure the relationships in general, but the relationships as influenced by the restriction of the tests to one half-hour in the case of (1) and (2) and the relationship as influenced by the variations in the degree of attention given to the words in scoring results in the case of (2). Moreover, I have not corrected the results for spurious correlation due to sex, nor for attenuation due to the small number of tests.

The lists used and the method of scoring were as follows: The number of individuals was 38 for relation 1 and 40 for relation 2.

The lists of words used were:

near	bell	break	maze	yet
out	cloud	call	crass	shall
false	box	sleep	hob	and
lot	slate	drop	zest	lest
gift	cap	smile	eke	how
end	wing	run	slink	could
cheat	flag	eat	fob	though
thought	bed	cry	lush	when
lose	stone	drink	elk	let
add	pig	hit	bland	your
queer	house	sing	tweak	since
full	nose	skip	lilt	more

The lists of numbers used were:

791	254	639	948	579
469	624	716	851	356

918	683	532	746	823
493	321	228	264	974
671	572	787	435	358

The score for one correct word was 1.

A word apparently misheard but remembered as heard (*e. g.*, slake for slate or amaze for maze) was scored correct. Each individual's testimony was accepted in such cases. For each word written that was not in the list a discount of one word was made. Such errors are rare, making only 3 per cent. of the words written; 50 seconds were allowed to write out the words remembered for each list.

Each three-place number recalled exactly counted 1. Each number of which two digits were correct and correctly placed counted .5. 30 seconds were allowed to write out the numbers for each remembered list.

The obtained 'raw' correlation for (1) is $.4\frac{1}{2} \pm .1$. The mixture of the two sexes and the testing of the two traits in the same hour tend to make this higher than the relation between the general ability to remember word lists and the general ability to remember three-place number lists. On the other hand, there is the attenuation due to the variation, in both (a) and (b), of the result from five tests from the person's true ability. I estimate that correction for all three would result in a correlation of about $.5\frac{1}{2}$. The relation between (a) and memory of lists of 12 single digits was found to be .6, eight independent records of each being used. Correction for attenuation raises this to .7—. So, until more adequate measures are made, we may accept as the most likely fact that, in such a test of brief retention, a variation in the content from words to numbers reduces the correlation from 1 to about $\frac{3}{4}$. Even if the reduction should prove to be to only $\frac{3}{4}$, the fact would still be very strong evidence of the dependence of efficiency of memory upon content and of the specialization of mental functions in general.

The obtained 'raw' correlation for (2) is $.5\frac{1}{2} \pm .1$. Allowing for the mixture of the sexes, the inaccuracies of the original measures, and the individual variations in the experiences upon which the memories for twenty-four hours were based, I estimate the relation as $.8 \pm .1$. I know of no other measure of the relation between brief and long retention in the case of unconnected material. Henderson, in the case of connected trains of thought, gives data for memory over a few minutes from three minutes' study and memory of the same material after forty-eight hours, based upon the three minutes' study and the experience of writing out what was remembered at its close. The resulting correlations would seem, if corrected for attenuation on the one hand, and for mixture of the sexes and of differently selected groups on the other, to be about .9.

The relation between retention of the effects of an experience for one or two minutes and their retention for one or two days thus seems to be one of the closest yet measured in human nature.

PROFESSOR WIRTH ON THE EXPERIMENTAL ANALYSIS OF CONSCIOUSNESS.¹

By L. R. GEISSLER.

The purpose of Professor Wirth's "Die experimentelle Analyse der Bewusstseinsphänomene," as set forth in the Preface, is to give an account of the present status of those part-problems of experimental psychology which Wundt had already formulated for the first time in 1874. This may account, to some extent, for the author's close adherence to the Wundtian system of psychology, a brief résumé of which is offered in the Introduction and the First Part of the book.

The Introduction deals with "Consciousness as a Natural Individual Unity" and begins with a brief sketch of "The Historical Development of the Concept." It then defines and discusses, in somewhat abstract and argumentative fashion, such points as self-consciousness, psychical causality, psychophysical parallelism, and mental dispositions, and concludes with an outline of "The Internal Organization of Consciousness and the Chief Kinds of Mental Contents." Here we meet with the familiar Wundtian dichotomy of *Vorstellungen* and *Gemütsbewegungen* as representing the objective and subjective phases of consciousness. The latter, again, are subdivided into states of passivity or *Gefühle*, with their positive and negative aspects, and into states of activity or *Willenserlebnisse*, with their blind or impulsive and their rational or voluntary aspects. Toward the end of this general outline (p. 17) the author says: "the concept of conscious content is in itself so general as to include the special characteristics of the higher mental functions, such as representation of an object, etc.; and since, in particular, the concrete content is never entirely void of the immediately experienced character of activity, it does not seem necessary to make any special distinction between 'contents' and 'acts.'" He thus takes sharp issue with one of the fundamental positions of the school of Brentano.

The First Part has for its subject-matter "General Considerations Concerning the Nature of Conscious Phenomena." The author informs us here that he means by experimental psychological analysis the attempt "to find, as far as possible, exactly measurable stimuli and unequivocally predetermined voluntary mental attitudes, as the basis for a universally comparable situation of consciousness" (p. 21). The conscious phenomena in the narrower sense, to which, if I understand him rightly, he intends to confine his analysis, are certain interrelations between concurring spatial and temporal complexes and their qualitative and quantitative aspects. The analysis of these phenomena would, however, be incomplete without considering their possible changes in degrees of consciousness. A complete psychological analysis is thus concerned, on the one hand, with a numerical or qualitative determination of the "Enge des

¹ *Die experimentelle Analyse der Bewusstseinsphänomene*, von W. WIRTH, etatm.-ausserord. Professor der Philosophie und Mitdirektor des Instituts für experimentelle Psychologie in Leipzig. Mit 27 Abbildungen im Text und auf einer Tafel. Braunschweig, Druck und Verlag von F. Vieweg und Sohn. 1908, pp. xiv, 449. Price Mk. 11.

Bewusstseins," and on the other hand, with a quantitative measurement of the degree of consciousness of simultaneous processes.¹ With regard to the latter problem, the author thinks that an immediate or direct estimation of the degree of consciousness by introspection is *a priori* doomed to failure, and for two reasons. First, the range of possible degrees of consciousness of a given mental process, with the one of its extremes bordering unconsciousness, "is something purely intensive, and therefore involves all the difficulties of the method of what is called supraliminal differences for intensities" (p. 33). The second and more important objection is the inconstancy of degree of consciousness in succeeding mental states, which destroys all hope of a successful application of the direct method. The actual reason, however, for Wirth's refusal to consider this possibility more in detail seems to the reviewer to be the fact that it had not received a place in the Wundtian programme. At any rate, recent experiments by the present writer with the method of direct estimation of degrees of clearness have shown that Wirth's *a priori* objections do not hold in actual practice. Their rebuttal in this place may therefore be omitted.

In speaking of degrees of consciousness it must be noted that the author distinguishes them sharply from degrees of clearness. "One may best understand the nature of the degree of consciousness by comparing the direct sense-perceptions with the reproduced ideas of memory and imagination referring to like objects" (pp. 34 f.). The difference between them lies in *Lebhaftigkeit* and *Frische*, vividness and freshness. The reproduced processes involve, as a rule, a much greater range of vividness and freshness, though in direct sense-perception, especially in the briefer processes, various degrees of vividness reveal themselves in the stages "des An- und Absteigens der Empfindungsfrische" (p. 35). In the case of the *Gemütsbewegungen*, this vividness manifests itself with great directness "as the real actuality of consciousness" (p. 36). Even so, the author does not commit himself to saying that degree of vividness and freshness is identical with degree of consciousness; they merely "stand in closest relationship to each other" (p. 36). In general, "the degree of consciousness stands so definitely at the centre of all psychical causal developments, that the significance of an element or of any abstract characteristic is always dependent upon it" (p. 36).

In our immediate experience the degree of consciousness reveals its effects in several ways. "The most immediate consequence of a high degree of consciousness in the case of a concrete idea consists primarily in the clear and distinct differentiation of the idea as a whole from the other mental contents, and of its parts and characteristics from one another, while a lower degree of consciousness corresponds to the opposite, an obscure intermingling and confusion" (p. 36). This clearness-effect is much less marked with emotional contents, as they are "very closely fused in content, and less clear than other equally vivid elements can be that are more sharply differentiated from their surroundings" (p. 37). But even in the case of ideational contents, changes in degree of consciousness are only approximately proportional to changes in clearness. Another important consequence of a high degree of consciousness manifests itself in introspection and memory. The degree of consciousness has a determining influence, *e. g.*, upon the duration "des Abklingens eines Inhaltes"

¹ "Das fertige quantitative Resultat der exacten Analyse wird sich also vorläufig immer aus der Angabe der inhaltlichen Basis in ihren oben beigezogenen psychophysischen Massen einerseits und des an jeder Stelle herrschenden Bewusstseinsgrades andererseits zusammensetzen" (p. 32).

(p. 39), or upon the psychophysical disposition for the reproduction of contents or of connection between contents, or upon the certainty and objective correctness of the various reproduced elements, their characteristics and relations. Finally, in a similar way, the range of symptomatic movements will, under simplified secondary conditions, correspond to the degree of consciousness of the feelings expressed by them, just as the course of external voluntary movements will be related to the degree of consciousness of the involved voluntary activity.

Now in every state of consciousness a purposive voluntary activity, a "zielbewusste Willenstätigkeit" (p. 41), is present in one or the other of its various forms. Attention, *e. g.*, is a form of voluntary activity whose complex of impulses aims at the greater clearness of certain ideational contents, especially of immediately perceived objects and events. Comparison, as another function of voluntary activity, "seeks to increase the degree of consciousness of a qualitative relation between contents" (p. 42). A general scientific term for all these forms is found in Wundt's concept of apperception. Under certain conditions, it is true, the apperceptive function of attention may fail to accomplish its aim in establishing a certain clearness-relievo, but it does not therefore lose its character as a voluntary activity. Under certain other conditions, again, certain mental contents may reach a very high degree of consciousness without any voluntary effort, but only on account of a momentarily high excitability of their underlying psychophysical dispositions. The nature and combinations of the particular impulses employed by apperception, and especially attention, for the sake of clarifying mental contents, depend entirely upon the special nature of the contents themselves. On the whole, these impulses work for an exact positive and negative (or inhibitory) adjustment of the body, and especially of the sense-organ involved. The impulses functioning in reproductive apperception manifest themselves, *e. g.*, in expressive movements and in symbolic tendencies of articulation, of writing, etc., but mainly in the internal apprehension and fixation of perceptions and thought-contents. Although an analytic study of the apperceptive activities can trace these impulses separately with more or less definiteness, yet in any concrete experience they co-operate as a whole, with more or less ease according to degree of practice, so that they may lead to a constant level of maximal apperceptive practice which is of cardinal importance for quantitative results in experiments on the degree of consciousness.

These are the general principles and considerations, according to which the author intends to conduct his analysis of conscious phenomena in the concrete, and which serve as his guide in the selection and treatment of the experimental literature. The rest of the work, from p. 56 to p. 443, is in the main a grand review—a connected presentation and a consistent interpretation in the light of the preceding discussion—of certain groups of psychological and related experiments. They are classified in Wundtian fashion as experiments by the impression-method, treated in the Second Part (pp. 56-340), and experiments by the expression-method, taken in the wider sense to include the study of symptomatic, ergographic, and reaction movements, treated in the Third Part (pp. 341-443). A list of nearly 400 names and authors referred to completes the work.

The Second Part discusses in its first half "The Analysis of a Single Moment," and in its second half "The Course of Continuous Conscious Events and the Time-Perception."

Several methods have been developed for the study of a momentary

consciousness. The first is based upon that effect of a high degree of consciousness which manifests itself in retrospection and memory. It employs the simultaneous exposure of several homogeneous stimuli, especially of the visual, tactual, and auditory kind, and presupposes the dual division of a mental state into focus and background, into a high level of relatively clear, and a low level of relatively obscure, mental processes. The problem is identical with the first part of the purpose of a complete psychological analysis, as described above, namely, the numerical or qualitative determination of the range of consciousness and, in particular, of attention; it has been attempted mainly for visual, tactual, and auditory sensations. The author goes into great detail with regard to the temporal relations of the stimulus-exposures, and his refreshing objectivity of treatment contrasts favorably with the abstract and generalizing language of the preceding introductory chapters.

The second method employed in the analysis of a momentary consciousness is called by Wirth the *Schwellenmethode* or the method of the difference-limen. It is based upon the other effect of a high degree of consciousness, namely, upon the clear and distinct differentiation of a given mental process as a whole from other simultaneous contents, and of its essential features or characteristics from each other. The clearness-effect is most nearly proportional to its cause, the underlying degree of consciousness, in the case of the singling out of a particular mental content from its simultaneous and successive neighbors in the same total perceptive complex. This condition is realized, in its most elementary form, in the determination of the difference-limen for a single characteristic, such as magnitude, position, brightness, etc. The unit for measuring the degree of consciousness of any of these characteristics is found in "the objective difference which evokes a judgment of difference of a determinate clearness and certainty" (p. 100).

In order to put this point more clearly, we may follow up one of Wirth's concrete examples. In a uniformly illuminated, monocular field of vision certain places or regions are so arranged that their brightness may be gradually and measurably increased until they are supraliminally different from the rest of the field. The whole unitary field is now exposed for a certain length of time and observed with a predetermined distribution of attention, *e. g.*, with attention to the right half of the whole field, but with fixation held always upon the same point at the centre. After a convenient interval, which is determined by the observer's attentive *Einstellung* or preparation, the field is briefly re-exposed with a certain change in the brightness of some (to the observer unknown) region. The change is gradually enhanced until it becomes just noticeable. It then constitutes the difference-limen for the region in question, and as such is the reciprocal value of the degree of consciousness pertaining to this particular region. A systematic application of the method, under various experimental conditions, and with distribution of attention either to the different characteristics of the total complex or to its various part-contents, requires that the observers be always able to give a constant amount of attention to the whole complex during both exposures without expecting a change in any particular direction. Even then it is possible that the various part-contents or characteristics depend, for their degree of consciousness, upon secondary phenomena; in vision, *e. g.*, upon the peculiarities of the periphery of the retina. It is therefore further necessary, in most cases, to determine the absolute difference-limen for every characteristic or every part-content with maximal attention to it itself, and thus to obtain

what is called the normal value of the limen. This is then compared with the difference-limen found with a given distribution of attention, and the ratio between the two values constitutes a more exact measure of the degree of consciousness for the particular characteristic or part-content in the original perceptive complex. The ratios thus found, for the various phenomena investigated, are reduced to the same base by making each normal value equal 1, so that the smaller ratios or fractions express the higher degrees of consciousness, and conversely.

The actual experimental application of such a method involves many details and complications, according to the sense-department and the nature of the perceptive complex selected. The field most extensively studied in this way is vision. Wirth discusses at some length the investigation of Mittenzwey, who worked out the degrees of consciousness for the characteristics of magnitude, position, and brightness of one and of several small white circles within a large gray circular field. In the case of one variable circle, with attention equally distributed over all three characteristics, Mittenzwey found that, if measured by the ratio between the normal value and the difference-limen, brightness required the highest degree of consciousness, .54, position demanded less, .67, and magnitude as expressed by decrease, .75, by increase, 1.00. This result shows that, in spite of the observers' intention to distribute their attention equally over all three characteristics, their effort is influenced by the relations existing between the phenomena observed. In the case of several small circles, with distributed attention, Mittenzwey found the following values expressing degrees of consciousness: .37 for magnitude, .36 for position, and .58 for brightness. If the observer concentrated his attention upon the same characteristic in all small circles, then the other characteristics did not suffer much loss in degree of consciousness; while, if all characteristics in one circle were attended to, the corresponding values for the other circles rose considerably, and in some cases their degree of consciousness was practically zero. An extensive distribution of attention shows, therefore, even in the case of one variable characteristic, the greater variety of degrees of consciousness. Its study was accordingly made the aim of Wirth's own experiments with the *Schwellenmethode*, which he very briefly reviews and summarizes. His perceptive complex was the uniformly illuminated field of vision of the left eye. It was divided into 37 regions, which were grouped concentrically around the point of fixation, and could be gradually and measurably increased in brightness. Since visual sensations change very rapidly in degree of clearness during the first 300 of their rise to consciousness (*Anklingen*), it was only necessary to know the time-interval at which an increase became just noticeable, and to use this as an equivalent of the visual difference-limen. Hence the formula for the calculation of the degree of consciousness pertaining to any one of the 37 regions was:

$$D. Cs. = \frac{\text{Intensity of } j. n. R\text{-increment} \times \text{Duration of } R.}{\text{Constant Initial Intensity of Region}}. \text{ In accordance}$$

with this formula the whole left field of vision was investigated, with the centre as constant fixation-point, but with attention distributed in the following different ways: (*a*) to the whole field, (*b*) to the left half of the field, (*c*) to the upper left quadrant, (*d*) to a region in the left upper periphery, and (*e*) to the fixation-point itself. Unfortunately, the results with these different distributions were obtained at various stages of practice, so that they are not comparable. Other obvious disadvantages of the method are the difficulty of maintaining a com-

plicated and constant *Einstellung* of attention which requires more than three months practice even for a skilled observer (p. 128), and the consequent difficulty of multiplying results with other observers and with variations of conditions. The main result of all these experiments is, according to Wirth, that the monocular field of vision shows at any moment only relatively small differences in clearness. In particular, under the most natural conditions, when the fixation-point is also the point of attention, the difference of average clearness for the five concentric zones is hardly appreciable. The average values, from centre to periphery, are: 1.16, 1.23, 1.23, 1.14, and 1.26. Wirth also applied his *Schwellenmethode* to touch, the pressure being varied intensively by means of von Frey's limen gauge. He used six pressure stimuli on the lower right and left arms, on the backs of the hands, and on the upper surfaces of the feet, and the obtained values were: for the right side 1.56, 1.96, and 1.10, and for the left side 1.36, 1.51, and 1.35. For tonal pitches he employed the *C* of 128 vs. with its first four overtones, all 5 tones produced by electric tuning forks. Their intensification was produced by movement of a noiseless cock in the tube conducting the tone to the ear. The values expressing degrees of consciousness for the four partials *C*, *c*, *g*, and *c'* were, for one observer, 1.31, 1.10, 1.16, and 1.16, and for another observer 1.16, 1.12, 1.14, and 1.02. In both instances, attention was distributed over the whole tonal complex.

In this connection Wirth devotes a few pages to the discussion of the method of measuring attention by distracting stimuli, and especially to its application by Peters, whose results, he thinks, are in general and essential agreement with his own.

Another and much more indirect way of determining degree of clearness makes use of its relation to accidental and constant errors of observation and comparison. (It will be noticed with some surprise that in the heading of this sub-division,¹ as well as in various passages throughout the text,² Wirth seems practically to identify degree of consciousness with degree of clearness.) Owing to the relative inconsistency of the psychophysical relation between stimulus and sensation, a given stimulus-difference may, under certain unfavorable conditions, remain subliminal, or a given stimulus equivalence may arouse a consciousness of inequality. The range of such accidental variations, or the mean error, is greatly reduced by a correct and "geschicktes Verhalten" (p. 151) of attention. To maintain such an attitude is, however, difficult, especially in the case of liminal determinations with distributed attention, where the characteristic whose limen is to be determined offers a constant point of attraction for attention. At the same time, expectation easily assimilates those characteristics most closely related to the one under observation, in favor of the clearness of the latter. Mittenzwey found, *e. g.*, mutually supporting assimilations between the characteristics lighter-larger-higher, and darker-smaller-deeper. Such interrelations occur most readily in the obscurer regions of consciousness.

Besides these accidental errors in liminal measurements there occur constant errors of assimilation which may be due, *e. g.*, to intentional distraction of attention during momentary exposures of objects compared, or to the associative influences of ideas. These last are especially strong in the illusions of perception, which there-

¹"Die indirekte Bestimmung des Klarheitsgrades durch seine Beziehung zu den Beobachtungs- und Vergleichsfehlern," p. 147.

²*Cf.* "Denn nach allen früheren Ausführungen stehen *Bewusstseinsgrade* oder *Klarheit* eines Inhaltes und Richtigkeit seiner repräsentativen Funktionen immer in enger Beziehung zueinander," p. 151. (Italics are mine.)

fore lead most frequently to wrong judgments, that is, to judgments of inequality with equal stimuli, and to judgments of equality with unequal stimuli. The extent to which these judgments depend upon the degree of consciousness of the secondary factors involved may be most easily determined, experimentally, by means of the optical illusions. It has been shown, *e. g.*, by Schumann, who made tachistoscopic exposures of the illusory complexes, and by Einthoven, who employed instantaneous illumination in order to eliminate eye-movements, that a lower degree of consciousness (conditioning what has been called an imperfect "apperceptive accommodation") exaggerates the Müller-Lyer arrow-illusion to a considerable extent. A similar effect can be obtained with longer exposures, according to Benussi, by a voluntary control of apperception. Benussi has further shown that, after practice, an apperceptive emphasis or disregard of these secondary factors increases or decreases the illusory effect. The apperceptive control of secondary factors is paralleled by the phenomena of fixation and of conscious, impulsive or voluntary eye-movements, whose influence is particularly shown in the illusions with depth-perception. Analogous phenomena occur with tactual illusions, which, on the whole, require no new principle of explanation. If, *e. g.*, attention is voluntarily directed first upon the place on the skin toward which the hand is moving, and then upon the various successive positions or attitudes of the moving limb, the constant error involved in equating distances or in localizing points upon the skin may become actually twice as large as if the attention is directed upon the act as a whole, so that each co-operating secondary factor can attain only to such a degree of consciousness as is necessary for a fairly correct judgment. The same principle is involved in the familiar phenomenon of breaking up an habitual action by attending to one of its components, a procedure which disturbs the clearness-distribution of the various contents; so long as they remain on a somewhat low, but more or less uniform level of degree of consciousness, their co-operation as members of the same complex is undisturbed. In this connection Wirth also discusses the temporal displacement of two simultaneous or almost simultaneous stimuli due to variously distributed attention. These phenomena may justly be called reversible illusions of the temporal order, and as such may be said to constitute a parallel to the reversible illusions of space-perception. Here again it has been found, by Hamlin, that a well distributed attention or "alert indifference" insures the largest number of correct judgments. One of the secondary factors here involved, but in Wirth's opinion not yet unambiguously described, is the previous voluntary *Einstellung* or vivid anticipatory imagination of either of the two possible temporal orders, or more particularly the rhythmical exaggeration of the imagined interval between the two stimuli. He thinks that a closer study of this factor may, *e. g.*, give a satisfactory explanation of the fact mentioned by Weyer, that out of the 12 possible combinations in pairs of light sound and touch, the two sequences light-touch and light-sound deviated from the general rule governing the magnitude of the difference-limen for temporal displacement.

This completes the discussion of the three methods used in the study of a momentary consciousness. It must, however, be added that clearness-distribution also depends, to some extent, upon the nature of the mental contents themselves, and upon their place in the mental pattern or complex under investigation. This dependence is brought out, *e. g.*, in the familiar psychophysical experiments that investigate the influence of varying quality or intensity

upon the absolute difference-limen with maximal concentration of attention. The most suitable material is again offered by space-perception, especially by the difference-limen for estimation of distances. The uniform proportionality which has been found to exist between given distances and the increments necessary to arouse a judgment of just noticeable difference may be most simply interpreted, according to Wirth, in the following way: "the clearness of every single 'element' involved in the comparison of two extensions and contributing to the consciousness of a total distance, is decreased in direct proportion to (the clearness of ?) the total magnitude. The maximal degree of consciousness necessary for a certain and correct recognition of an objective difference can therefore be obtained only by corresponding enlargement of the absolute difference" (p. 204).¹ The influence of independent mental processes upon the clearness of other independent processes has been shown in the experiments of Sante de Sanctis and of Heymans, and in various investigations of distraction of attention. It has also been studied quantitatively by Benussi, who utilized the errors of comparison in the geometrical-optical Zöllner illusions, with variously colored main and secondary lines.

This mutual influence is removed, if the competing mental processes occur successively at such intervals as by trial are found to be most favorable for its elimination. Furthermore, the mutual disturbance of co-exciting mental processes is sometimes more or less counteracted by the fact that the presence of a rival acts as a sort of spur for attention to the primary stimulus. Finally, it must be noticed in this place that a simultaneous execution of two disparate activities sometimes leads to a change of the one of them from its habitual to a newly-elaborated performance; the recitation of poetry, which by itself takes places as a rule in acoustic-motor terms, may be mainly carried on, if performed together with mental arithmetic, in visual-motor terms.

In the following subdivision of the Second Part the author takes up "The Course of Continuous Conscious Events and the Time-Perception."

The mutual interrelation of simultaneous mental processes is represented, in its most concrete form, not in the momentary cross-section of consciousness, but in the longitudinal division which permits the study of a given mental content during a certain interval of time. The necessary conditions for analysis are, aside from the frequent repetition of the identical complex, voluntary apperceptive activity and the continuity of the content. They are realized, *e. g.*, in the experimental studies of mental work, begun by Kraepelin for the adult consciousness and by Burgerstein for the child's mind. Even the interference from such factors as fatigue, distraction, dislike, etc., serves only to make the general conditions more concrete, that is, more like those met with in daily life. On the other hand, it will ordinarily be necessary to eliminate the observer's time-sense, as an indirect motive to his apperceptive activity, by requiring him to abstract from the idea of time, and by objective control and variation

¹ Sie (eine ziemlich gute Proportionalität des absoluten, eben merklichen Längenunterschiedes zu der Gesamtstrecke) scheint aber nun am einfachsten so gedeutet werden zu können, dass die Klarheit jedes einzelnen an der Vergleichsrelation beteiligten "Elementes" der aufeinander bezogenen Extensionen, das zum Bewusstsein einer Gesamtstrecke beiträgt, direkt proportional zur Gesamtquantität herabgesetzt wird. Der maximale Bewusstseinsgrad, welcher zur sicheren und korrekten Erkennung des objektiven Unterschiedes notwendig ist, kann daher erst einem entsprechend vergrösserten absoluten Unterschiede zukommen."

of the temporal conditions. The quantity and quality of the mental work performed under such conditions are a direct and precise measure of the existing degree of consciousness; a measure which, if necessary, may be refined by use of the error-methods.

When this method is applied to the continuous observation of liminal stimuli, it leads to the well-known phenomenon of fluctuation of attention. In reviewing the literature of this problem, Wirth arrives at the conclusion that the fluctuation of liminal auditory sensations is certainly not due to peripheral adaptation. The latter is, however, much more important for vision. Nevertheless, the great irregularities in visual fluctuations, their frequent coincidence with circulatory and respiratory oscillations, and their similarity to fluctuations of supraliminal intensities, are strong arguments in favor of a central seat of the fluctuation.

The study of the Time-Perception itself contributes to the solution of the general problem of the experimental analysis of mental phenomena two important points. The first concerns the development of the individual temporal idea. We have here the factors (*a*) of temporal position or *Zeitlage*, best represented in consciousness by the briefest kind of impression, (*b*) of duration, (*c*) of articulated succession, and (*d*) of empty intervals or pauses. All of these may occur with the most varying degrees of consciousness, and in this respect they are analogous to the factors involved in space-perception. The methods of measuring their degrees involve comparison of two successive time-intervals, the one of which as a rule must be constant, while the duration of the other may vary. It is impossible to enter here upon a detailed review of Wirth's discussion of the various investigations of the time-sense, and of such related phenomena as rhythm, continuous change of sensation, the question of "sensed time," temporal or stroboscopical illusions of moving and resting objects, and the subjective division of time-intervals into equal temporal distances.

The second contribution to be mentioned is the result of the complication-experiments originally derived from the astronomical "eye and ear method." A precise and exhaustive analysis of the factors determining the limen of temporal coincidence and displacement and the magnitude of the errors involved can be secured only by the application of the method of minimal changes, without knowledge on the part of the observer. The liminal values thus found, under varying experimental conditions, are on the whole in agreement.¹¹ Greater importance, however, attaches to the magnitude and direction of the error resulting from the variation of conditions. With a practically uniform distribution of attention over the two coincident mental processes, the error of displacement is small and as often positive as negative. The same result is obtained, still more clearly, by systematic and skillful practice and general mental alertness. These correcting influences are counteracted by such factors as the regular repetition of the experiments, which demands a more or less rhythmical *Einstellung*, the direction of the moving hand on the dial of the complication clock, and the introduction of more than two simultaneous, similar or disparate stimuli. The explanation of the fact of displacement was first given by Wundt, and Wirth merely restates it in less clear language.

The Third Part, finally, discusses the experimental methods of expression, used especially in the investigation of the affective and volitional aspects of mental life. There are mainly three of these. The first is termed the symptomatic method, and deals with those involuntary innervations and secretory processes of the body which

indicate or accompany emotional and volitional states of consciousness. The symptomatic factors expressing, *e. g.*, excitement and depression, strain and relaxation, pleasantness and unpleasantness, arise out of complex mental contents which for the moment occupy mostly the vague background of consciousness, while the impulsive factors of a voluntary action belong to those clear contents which form the sharp, tapering parts of a clearness-relievo. The interrelations of these two kinds of factors are very intricate, and are seemingly complicated by the facts of sympathetic co-excitation, or of competitive inhibition of such bodily functions as respiration, circulation, etc. In so far as the co-ordination between the mental states and these physiological factors is established, the observations and measurements of the latter allow us to draw inferences from them concerning the degree of consciousness of the affective and volitional factors whose expressions or accompaniments they are.

The second method, especially suitable to the analysis of the volitional aspect of mental life, is the ergographic. Like the former, it has a physiological origin and parallel, in the experiments on the tetanised muscle-preparation which naturally suggested experiments on the living muscle, and developed for that purpose a long list of such apparatus as ergographs, dynamographs, dynamometers, etc. One great advantage of this over the previous method is the direct introspective control of the voluntary effort exerted under given experimental conditions. Corresponding to the co-ordinated physiological factors entering into the results of the previous method, the ergographic method in its turn has to consider the factors of fatigue and practice as well as those of co-fatigue and co-practice of the muscular system involved, and their relation to the volitional factors. The work performed by certain muscles of the body, under a constant voluntary impulse, is measured by the instruments employed and indicates the degree of consciousness of the effort exerted. These results may be affected by other simultaneous mental processes as well as by simultaneous work of a secondary kind. The co-ordination of mental and muscular work is of especial importance for the analysis of the volitional aspect of consciousness.

The third method, finally, is based upon experiments with reactions in the narrower sense. Wirth's general discussion of the components of consciousness in simple and choice reactions, and of their systematic control, does not present anything novel or strikingly different from Wundt's exposition in the 5th ed. of the *Grundzüge der physiol. Psychologie III*. In the last section of the book, however, Wirth takes up the reaction-experiments conducted by himself and Kästner, in which "the clearness of the stimulus motive or the vividness of the presentation of the selected impulse" is measured by the quotient of reaction times obtained under complex and under simple conditions. But a comparison of the results of the simple reactions with the corresponding results of the *Schwellenmethode* shows that an approximate similarity obtains only during an intermediate stage of practice in reaction, while after greater practice (of about five months) the reaction times with different distributions of attention no longer portray any typical distribution of degrees of consciousness in the general clearness-relievo. This incongruence, Wirth thinks, may be explained by assuming that the time-differences in the reactions are not due to gradual change from minimal to maximal clearness of the perceived stimulus as such, but result from the clearness with which a given stimulus is *apprehended as a motive to reaction*. The hypothesis seems to be supported by the results of the choice or 'disjunctive' reactions, in which the one hand was co-

ordinated with the part of the visual field attended to, while the other reacted to changes in the part not attended to. The retarded reaction times for the latter were due to the delayed co-ordination of the stimulus as a motive to the correct hand.

This account must suffice to serve as an indication of the main contents and chief points of interest of a book which is as rich in detail as it is broad in scope. The interweaving of observation and generalization would only increase the value of the work, if the author's style were less involved and less abstract. As it is, the combination greatly enhances the difficulty of understanding a work whose language is often hopelessly confusing. The author sometimes repeats himself unnecessarily, thus loading his sentences with cross-references and obscuring their grammatical construction; sometimes he coins terms, to be employed in a very technical sense, without expressly defining them; and sometimes he uses familiar words in a very specific connotation without further explanation. For these reasons, the reviewer finds it difficult to enter upon a detailed criticism, as he is unable to judge in how far he has correctly understood and interpreted the work. He disagrees, in general, with the author's definition of degree of consciousness and his method of measuring it, with his treatment of the fluctuations of attention, with his discussion of the nature of apperceptive and attentional activity, and with the general presentation of the emotional and volitional aspects of mental life. The first two points of difference concern matters of fact, which have to be decided by experimental method; the two last are still too closely bound up with theory and hypothesis to allow of the hope of satisfactory solution in the near future. The reader who, for the time being, accepts Professor Wirth's point of view will find the author's presentation and interpretation of his subject-matter thoroughly systematic and consistent. While his standpoint is essentially Wundtian, he has also incorporated various Lippsian elements and even uses terms which remind one of the Austrian school. His knowledge of the history and literature of the science makes his treatment of its experimental development especially valuable, and his work will therefore be appreciated by many as a source-book of psychological methods and references.

PSYCHOLOGICAL LITERATURE

✓ *An Arraignment of the Theories of Mimicry and Warning Colors.* By ABBOTT H. THAYER. Reprinted from the *Popular Science Monthly*, December, 1909. pp. 550-570.

Concealing Coloration in the Animal Kingdom: an Exposition of the Laws of Disguise Through Color and Pattern. Being a Summary of Abbott H. Thayer's Discoveries. By GERALD H. THAYER. With an Introductory Essay by A. H. Thayer. Illustrated by Abbott H. Thayer, Gerald H. Thayer, Richard S. Meryman and others, and with photographs. New York, The Macmillan Co., 1909, pp. xix, 260. Price \$7.00 net.

Modern biology has occupied itself, in great detail, with the problems of organic coloration. The main field of discussion has naturally been that of the surface colors of animals; for internal and invisible coloration can have no biological significance, and "the dominant coloring of plants is an essential element in the paramount physiological activity of chlorophyll" (Poulton). In general it is held that the visible colors of animals, and in exceptional instances those of plants, have, for immense periods of time, been modified and arranged to assist in the struggle with other organisms or in courtship. There are, first of all, the phenomena of protective and aggressive resemblance, colors which enable an animal to conceal itself from its enemies or to approach its prey unseen, procryptic and anticryptic colors. There are warning colors and recognition marks, aposematic and epise-matic characters. There is the great mass of observed facts grouped under the head of mimicry: Batesian or pseudaposematic mimicry, an advantageous or deceptive resemblance borne by palatable or harmless species (the mimics) to others that are unpalatable or otherwise specially defended (the models); and Müllerian or synaposematic mimicry, the advantageous adoption of a common advertisement by specially defended species, whereby the loss of life incurred during the education of inexperienced enemies is contributed jointly by the similar forms instead of by every species independently. And, finally, there are the epigamic characters, explained by Darwin's theory of sexual selection. Color, at first incidental or non-significant, is thus supposed to have furnished admirable material for selection, natural and sexual, and to have played a large part in the mechanics of organic evolution.

With the central doctrine, that surface color is of high importance in the struggle for life, the author and inspirer of the two works under review has no quarrel. On the contrary, he very strongly insists on the adaptive character of coloration, of color itself and of color-pattern. He assumes the truth of natural selection; he believes, with Darwin, that natural selection operates upon small chance variations; he emphasizes the value to a species of adaptive preparation for such circumstances in its life as are of only occasional occurrence. But he insists, just as strongly, that expert judgment in the matter of the importance of coloration lies, not with the biologist, not even with the field-biologist, but with the student of color, the artist and the psychologist. Biologists have, with a few salient exceptions, appraised the conspicuousness or inconspicuousness of color and pattern from

the isolated specimen and, what is equally disastrous, from the human point of view. The right method is to set the animal in its natural surroundings, to bring its coloration and movement into relation with the colors and changes of its background; and then, having done this, to contemplate the animal from the point of view not of man but of those other animals which, whether as enemies or victims, have an interest in its discovery. If these rules are followed—and they surely need no defence—we have a surprisingly simple and uniform result: all creatures that ever prey or are preyed on prove to be colored in the manner best calculated to conceal them at the moments of their greatest need.

At first reading, this rule seems far too simple to be true; and as one reflects upon it, a large number of what seem to be obvious exceptions present themselves to the mind. Mr. Thayer has, therefore, done well to take the line followed in his book; to give his readers a great array of ocular demonstrations, clear of theoretical discussion. But he has also done well to preface the book by the article in the *Popular Science Monthly*, and to formulate his negations before he offers the positive testimony for his position. The article is intended to demonstrate the fallacy of the badge and warning color theories; and though it is not, in the same sense, an attack upon mimicry, it inevitably suggests that this theory cannot survive the demise of the others. Like the book, it has for its backbone a series of plates that demonstrate, beyond dispute, the primal effect of patterns and the wrongness of the older hypotheses about them. It begins by pointing out how "Darwin's erroneous supposition that a conspicuous mark on an object makes the object itself conspicuous has been built on and rebuilt on by the leaders of zoölogical research, even down to the present day. Entomologists, especially, make much of the supposed power of sharp and strong patterns to render conspicuous that particular part of the insect which they occupy." A fairly elementary consideration of the laws of irradiation, contrast, induction, as they operate under the conditions found in nature, is enough to prove that the effect of these patterns is the very opposite. Save for the comparatively rare moments when monotone exactly matches its background, pattern in aerial animals works for concealment in direct ratio to its own conspicuousness and elaboration.

The limits of space forbid my following the writer into his secondary arguments against warning colors and recognition marks; the fundamental arguments are, as I have said, furnished by the plates themselves. It must suffice to say that they are concerned in part with the intrinsic improbability or incoherence of the underlying theories, and in part with alternative explanations based upon the laws of visual perception mentioned above. The general argument against mimicry runs as follows: "As a few hours' experimenting in obliteration by juxtaposition of patterns will prove to any student, the optical laws which govern it are so absolute that one is not surprised to find that the whole world's butterflies have scarcely three different schemes of pattern. The principle of pattern arrangement in the famous 'mimetic' groups is out and away the predominant one over the whole globe. If this is the case, is it strange that in each most swarmingly populated seat of butterfly life there prove to be a number of species which, living in the very same station, and with seemingly identical habits, have, in obedience to this great pattern-law, practically identical patterns and form? We see in the ocean, for instance, even mammals wearing the shape and color of fishes." If this argument is sound—and Mr. Thayer states that he has found no exceptions at all, after seven years of almost continuous investigation of the pat-

terns of the world's butterflies—Professor Poulton's occupation is gone. It is a little curious that Poulton himself, while he recognizes the principle of counter-shading and has even discovered its dynamic operation in the Chamæleon, apparently fails to see the danger which threatens his favorite studies. Yet Mr. Thayer's law, if it be established, makes the tracing of mimetic resemblances at best a mere side-issue, and at worst a futility.

It is to the establishment of the law that the book before us is devoted,—surely, for those who are interested in general biology, one of the most interesting books of all the recent crop. One naturally turns at first to the colored plates: to the Peacock amid foliage, the Ruffed Grouse in the forest, the Cottontail among ferns and moss and grasses. It is unnecessary to say that the creatures are admirably pictured, but it is pleasant to be able to add that the mechanical reproduction is satisfactory. These illustrations are convincing, as regards their special subjects; but their effectiveness is increased by another group of plates, in which the argument that they embody is generalized,—the plates showing drakes of the Wood Duck in the water, Blue Jays against snow, Birds of Paradise in the tropical forest, caterpillars of various species on leaves and twigs; and in particular, the plates which place a line of Spoonbills or Flamingoes against the dawn or sunset sky. These plates are nothing less than illuminating; and the Publishers' Announcement is keeping to the literal truth in its statement that "the colored plates anticipate the future of wild animal illustration, showing, for the first time, concealing colors *in operation*." The picture of the Copperhead Snake caps the climax; the animal lies on a bed of dead leaves, and the plate is faced by a blank page from which its outline has been cut. Seen on the white ground, the animal is obvious, of course, by form and color; seen on the background of leaves, it disappears, and even a close scrutiny can trace its contour only with difficulty and by repeated reference to the blank form. All this must be seen to be believed.

There are sixteen colored plates, and there are a hundred and forty figures in black and white,—models, diagrams, photographs from nature. Here is the array of ocular demonstrations, upon which the thesis of the book mainly rests. That thesis is, to repeat, that "the entire matter [of animal coloration] has been in the hands of the wrong custodians. . . . It properly belongs to the realm of *pictorial art*, and can be interpreted only by painters. For it deals wholly in optical illusion, and this is the very gist of a painter's life. . . . This book demonstrates that the colors, patterns, and appendages of animals are the most perfect imaginable effacers under the very circumstances wherein such effacement would most serve the wearer, . . . the most gorgeous costumes being, in their own way, climaxes of oblitative coloration scarcely surpassed even by moths or inchworms. . . . The means of objects' recognizability, no matter how they are colored or marked, is almost always their *silhouette*—*i. e.*, their outlines in 'relieving' darker or lighter or differently colored against their background. . . . Patterns on animals' coats are the utmost that Nature can do in opposition to these potent vicissitudes of silhouetting." As regards terminology, Mr. Thayer recognizes Oblitative Coloration and Mimicry as the two main principles of Protective Coloration. The former principle covers all those concealing colors which serve to render the animals invisible in their native haunts. It is mainly based on counter-shading,—a discovery which was originally published in *The Auk* in 1896, has often been discussed and illustrated in recent years, and may therefore be assumed to be familiar to the reader. The second principle covers all those colors which

make an animal look like something else than what it really is; mimicry, that is, aims at deceptive visibility, while obliterative coloration aims at invisibility. It is, perhaps, regrettable that the writer uses mimicry in this wide sense, now that its use has been definitely restricted in current biological discussion; obliterative coloration might better be paired with, say, obliterative resemblance. However that may be, mimicry plays but a small part in the work; only a few examples are given, and these chiefly from among the lower orders; in the higher orders it has, in Mr. Thayer's opinion, a very secondary importance.

After the introductions—general by Mr. A. H. Thayer and special by Mr. G. H. Thayer—comes a series of seventeen chapters, dealing mainly with the markings of birds; three chapters are then devoted to mammals, one to fishes, one to reptiles and amphibians, and three to insects and spiders. It is difficult, where so much is given, to make a selection. But, if a choice is to be made, we may perhaps begin with the skunk, to which a good deal of attention is paid both in the *Popular Science Monthly* article and in the book. Belt, in *The Naturalist in Nicaragua*, tells us that at night "the skunk goes leisurely along, holding up his white tail as a danger-flag for none to come within range of his nauseous artillery." He describes the large white tail, laid over against the black and white body, as producing a very conspicuous effect in the dusk. There could not be, according to Mr. Thayer, a better instance of the naturalist's fallacy; the skunk's markings are beautifully obliterative. First, the mixed black and white of the body cause the animal to fade from sight at a short distance, and may even, at a near view, confuse themselves with details of forest and shrubbery. Secondly, a night is rarely so dark that a solid form within a foot of the eye would not show dark against the sky or against the light parts of the forest ceiling; hence, the white forehead markings, borne by the skunks and other grubbers of small surface life; the white markings counterfeit the sky, and are thus, as it were, seen through, as if transparent. Thirdly, the animal's white top has a cardinal function to perform; it effaces his upper contour against the sky to inhabitants of the turf; it too, therefore, is functionally transparent, an open space in the depths. And lastly, the skunk's tail is normally a mixture of black and white hairs, like a gray cloud. These points are clearly shown in Figs. 4 and 8-12 of the *P. S. M.* article, and in Figs. 95-103 of the book. The compound picture of Fig. 103 may, indeed, be said to furnish a key to the whole volume.

For a second instance, we may take the coloration of the Blue Jay, whose entire costume proves to be an arrangement of the brilliant colors of sunlit winter scenery,—his white the sunlit snow, his blues the varying shadows, his black the tree trunks and bare twigs, his ashy sides the haze of winter bushes. A glance at the plate facing p. 107 will train the reader's eye for observation of these facts in the open.

Lastly, a word may be said on nuptial colors. These, Mr. Thayer insists, are not to be ascribed to sexual selection, nor do they serve to render their wearers conspicuous. On the contrary, they represent plainly an increase of such potency of obliterative coloration as belongs to all gorgeously varied costumes, and this at the very moment when concealment is most needed. The puffin's bill, as he stands on guard at the mouth of the nest-burrow, serves by its gaudy color to obliterate the dark mouth of the hole, and at the same time substitutes a semblance of flowers to complete the deception. The moment his domestic duties are over, and he is back in the open sea, we behold

the bird dressed again in the universal ocean-and-rock colors of his habitat.—

Mr. Thayer's indictment of the naturalists may, if I have understood him aright, be summed up in four propositions. They have failed to realize that conspicuousness is solely a matter of relation to background; that black is conspicuous against white, and white against black, but that neither is conspicuous in and for itself. They have forgotten that there may be a vast difference between the conditions under which they see an animal and the conditions under which he is present, in nature, to his enemies or victims; so that his conspicuousness to human eyes may be altogether irrelevant. They have assumed, without test, that patterns make their wearers conspicuous. And, rightly noting the fact that aerial species in general are relatively conspicuous, they have failed to inquire whether any change of costume would remove or lessen this source of danger. In support of his own view—less, perhaps, a view than a discovery—Mr. Thayer offers a multitude of actual observations, covering all the main divisions of the animal kingdom. The labor involved in these observations has been immense, but has evidently been a labor of love. As such, it is its own reward; though external reward will also come in the conversion of many readers to the painter's, or, as I should prefer to say,—if this change of terms may be made without injustice,—to the psychological standpoint. No doubt, there remains the great and ultimate question of causation; but that remains for the followers of Bates and Müller no less than for Mr. Thayer.

E. B. TITCHENER.

Handbuch der Schwachsinnigenfürsorge mit Berücksichtigung des Hilfsschulwesens, Herausgegeben von Hans Bösbauer, Leopold Miklasu. Hans Schiner. Wien, Karl Graeser & Kie, 1909.

The following are the chapter headings: I. The nature of Feeble-mindedness. II. The kinds of Feeble-mindedness. III. Symptoms of feeble-mindedness (a) Bodily (b) Psychic. IV. Causes. V. Measures for prevention. VI. History of the movement for care of the feeble-minded. VII. Education and treatment of the feeble-minded. VIII. Forms of education. IX. Organization of institutions and "Special classes" (Hilfsschulen). X. Instruction. XI. Personality of the teachers. XII. After-care for the children who go out from the institutions and classes. XIII. Legal protection for, and military service by, the feeble-minded. XIV. Bibliography.

As is evident from the contents, the subject is discussed in many phases. The writers have gone over the literature with great thoroughness and have brought together many important facts and opinions. The book is conservatively written and gives no new data nor takes any advanced stand on any of the important questions involved. Most of the opinions quoted are given without criticism. The usual "causes" are given and discussed without much consideration of their relative importance. *E. g.*, alcohol is taken as a serious matter and important cause. No hint is given that any one doubts that it is an important cause.

In discussing "prevention," most everything is spoken of except the regulation of marriage—which would probably stop 80% of the trouble. Mention is made in fine print of a "strange kind" of law that has been passed in Indiana (the law authorizing castration). That is the only reference to that important matter. In discussing symptoms and education and treatment, the authors labor under the difficulty of too few heads of classification. "Feeble-mindedness" covers a wide range of conditions. It is not possible to discuss the

symptoms of feeble-mindedness in any helpful way without specifying many degrees of defect. The same is true of the education and treatment. The authors do make use of the term "idiot" for the lowest grade, but even then there is too much variation in what is left.

But this is a difficulty that we all labor under more or less. Three groups are fairly well marked out, but beyond that there is not much unanimity in the classification.

The book is well written and is a great contribution to our literature on the subject.

The bibliography is very extensive and a valuable adjunct. Its value would have been still greater if it had been classified or supplied with a subject index. The book ought to be translated into English. Our literature, that is available for any but the specialist, is far too scanty.

HENRY H. GODDARD.

The Survival of Man; a Study in Unrecognized Human Faculty. By Sir OLIVER LODGE, F. R. S. New York, Moffat, Yard & Co., 1909. pp. viii, 361. Price, \$2.50 net.

In this volume, Sir Oliver Lodge illustrates, by reference to investigations pursued during the past quarter of a century, the manner in which his own conviction of man's survival of bodily death has been acquired, and the kind of evidence by which he believes that this conviction will in due course be scientifically justified. The investigations which he reports deal with the phenomena of experimental telepathy, of spontaneous telepathy and clairvoyance, and of automatism and lucidity; an account of his experiences with the controverted and often discredited 'physical phenomena' associated with exceptional mental states is, the Preface states, reserved for another volume.

The book is frankly popular in its appeal, and contains nothing that is new to a student of the subject. Thus, the Introduction reprints the author's address delivered as president of the Society for Psychical Research in 1903; the evidence for telepathy is reprinted from the *Journal* and the *Proceedings* of the same Society, or from such well-known sources as Myers' *Human Personality*, and the *Phantasms of the Living*; and so on throughout. The writer's reliance is on the cumulative character of the testimony. No doubt, there are many readers who will be impressed by this feature; but there will be others, of stiffer mental fibre, who will insist that a chain is no stronger than its weakest link, and that, as every one of the links here passed in review has its weak place, the assemblage has no great claim to scientific consideration.

The discussion, as we should expect from the author's general reputation, is within its limits fair and candid. It is a question, however, whether the limits themselves should not, in the interests of science and truth, have been extended. Sir Oliver Lodge might, for instance, have inserted a chapter on the range of the unconscious whisper, on the indicativeness of unconscious head-movements, on muscle-reading, and on the various forms of experiment upon normal suggestibility published in the various psychological journals. The facts and conclusions of such a chapter would, surely, not have been out of place; it is a canon of scientific procedure to furnish negative as well as positive instances. The author is probably unaware of much of the work done upon these topics, in recent years, by experimental psychology. At the same time, its inclusion would have rendered his book less one-sided than it now appears.

FRANCIS JONES.

The Right to Believe. By ELEANOR HARRIS ROWLAND. Boston and New York, Houghton Mifflin Co., 1909. pp. xv, 202. Price \$1.25 net.

The writer of this little book, who is an instructor in philosophy and psychology at Mt. Holyoke College, explains its purpose and origin as follows. "I have happened to engage in discussion with some twenty persons, whose minds were more or less at sea in religious matters; and while their starting-points were different, their difficulties fell into somewhat well-defined types. The method which I suggested for answering certain of these questions was an apparent help in enough cases to justify me in thinking it could stand the test of a wider audience. To any criticism of the book (and there may be many) I can always reply: It has answered the questions of a dozen people. I claim for it nothing more." The book itself falls into six chapters, which discuss in a vivid and interesting way, though in every case with a foregone promise of orthodox issue, the necessity for a belief, the existence of God, the nature of God and of man, the divinity of Christ, the problem of evil, and prayer. Miss Rowland makes brilliant use of her philosophical and psychological knowledge; she meets the 'plain man' on his own ground of common sense logic, and beats down his scruples and objections with weapons like his own; her special pleading is concealed, partly by rhetorical skill, partly by intensity of personal conviction. She has, if the reviewer is not mistaken, a real gift of exposition in popular philosophy; and her greatest danger, if he reads her work aright, is facility.

W. JENKINS.

Spinoza's Short Treatise on God, Man and Human Welfare. Translated from the Dutch by L. G. Robinson. Chicago, Open Court Publishing Co., 1909. pp. xxiv, 178. Price, cloth, \$1.25 net; paper, 50c.

Students of philosophy are under a very real obligation to Miss Robinson for this translation of the Short Treatise—the first English version of Spinoza's first philosophical work. The translation has been made from the Dutch text of Van Vloten and Land, with frequent reference to the German of Schaarschmidt. It is, perhaps, rather over-literal; but in general, so far as the reviewer can judge, it is accurate and dependable. Blemishes of style and possible slips in rendering can, at all events, be corrected by the instructor in the classroom and by the translator in subsequent editions. Whether it was worth while to reprint, as introduction, Schwegler's chapter on Spinoza is, perhaps, questionable. Our dominant feeling should, however, be one of sincere gratitude to translator and publisher for making the Short Treatise accessible to our students.

JAMES FIELD.

Die Mechanik des Geisteslebens von MAX VERWORN. Teubner, Leipzig, 1910, 2te Aufl. pp. v, 114.

The first edition of these lectures upon fundamental problems of physiological psychology, by one of the foremost of German physiologists, has already received notice in this *Journal* (XIX, 1908, 266 f.). The second edition differs from the first only in a slight expansion of the text at certain points and some increase in the number of illustrations.

E. C. S.

BOOK NOTES

Könstlivet i Opdragelsen. P. F. LANGE. 2. Oplag, Aarhus, 1909. pp. 78.

This little work on "The Sexual Life in Education," is by one who has had some seven years' experience as a teacher in Denmark. The author, now a school-inspector, has been professor in Sorö College and also Vice-President of Marselisborg College. Professor Lange's chief conclusions are as follows:

1. Lying and fabrication are absolutely evil here. Children ought to have open and truthful information from their parents, as soon as their questions are asked out of real interest.
2. At school the text-books used by the children in natural history ought to give the necessary data concerning sexual conditions.
3. Co-education of boys and girls is a hindrance to really intelligent treatment of this question.
4. There exists no proof whatever that the healthy child can incur serious injury to his physical or his mental health through occasional masturbation.
5. There is abundant, irrefutable proof that children, who have been frightened by exaggerated and untruthful stories of the evil effects of masturbation have from the fear induced suffered greatly both in bodily and in mental health.
6. The most dangerous and most injurious effects are observed on those occasions when the child is suffering simultaneously from sexual and religious afflictions.
7. The chief task of parents is preventive. It consists principally in giving the child reasonable physical and mental instruction. If there is direct interference, it must be with caution and gentleness.
8. It is most important that the child should not make to others, and particularly not to himself, promises which he cannot keep.
9. The school has, in relation to the question of masturbation, no direct task.
10. When boys go out into practical life, they ought to be given, either on the part of the home or on that of the school, full information as to the ease with which sexual disease may be acquired and the great dangers therein both to themselves and to others.
11. First and last, meanwhile, come the relations of trust and confidence between parents and children. Not glittering, waste moral preachings, but life lived together with children and for them.
12. A natural consequence of the commandment, "Thou shalt honor thy father and thy mother," is, "Thou shalt honor thy children."

A. F. C.

The Sexual Life of Our Time: its Relation to Modern Civilization.
By IWAN BLOCH. Translated from the 6th German edition by M. Eden Paul. Rebman, Ltd., London, 1909. pp. 790.

The author's aim in writing this book was to compile a pretty complete encyclopedia on the sexual sciences. It certainly is the most comprehensive work on the subject which is extant in English, treating nearly all the various aspects of it and with remarkable plainness and detail. The author does not address himself to special topics and is therefore less exhaustive upon these than writers like Havelock Ellis.

Pubertät und Schule, von A. CRAMER. B. G. Teubner, Leipzig und Berlin, 1910. 16 p. (Schriften des deutschen Ausschusses für den mathematischen und naturwissenschaftlichen Unterricht. Heft 4.)

The chief thesis of this work is that the school does not exist to benefit psychopathic individuals, but that teachers of the future must be more or less acquainted with the clinic of puberty, in order to understand their children in general during their most critical period of life, and in order to distinguish between what is normal and what is abnormal in them.

Studies in the Psychology of Sex, by HAVELOCK ELLIS. Volume 6, *Sex in Relation to Society*. Davis, Philadelphia, 1910. 656 p.

In his previous five volumes of studies, the author has dealt with the sex impulse mainly in relation to its object, leaving out of account the external persons and the environmental influences. In this volume he now considers its relations to society and discusses in a more summary way the manifold, important problems that are presented. The previous work entered a more or less neglected field, which required analytic care and precision; but the ground covered by this volume is more worked. It treats of the mother and her child, sexual education, nakedness, evaluation of sex love, the function of chastity, the problem of abstinence, prostitution, the conquest of venereal diseases, sexual morality, marriage, the art of love and the science of procreation.

Das sexuelle Problem und seine moderne Krise, von E. MERTENS. M. Kupferschmid, München, 1910. 474 p.

The author first treats of propagation in animals and plants, of the human organs, of pre-marital hygiene and regimen for boys and girls, the ethnology and early history of marriage, sexual hygiene within it, prostitution, diseases, abnormalities, stages of development, with final sections on the right of motherhood, free love, flirtation, emancipation of women and sexual education.

✓ *The Sexual Life of Woman*, by E. HEINRICH KISCH. Authorized translation into English by M. Eden Paul. Rebman Company, New York, 1910. 686 p.

The writer treats first the sexual epoch of the menarche—its retardation, acceleration and its pathology generally, the causes of the determination of sex, fertility, sterility, various abnormalities, the age of senescence—its normality and pathology. The work is enriched by ninety-seven cuts.

Die Syphilis der Unschuldigen (Syphilis Insontium), von OSKAR SCHEUER. Berlin, Urban & Schwarzenberg, 1910. 239 p.

Although the author is a professor of sex and skin diseases, his book has a certain pathos for it is devoted largely to a description of cases where syphilis has been pandemic, that is, epidemic or endemic, and where it has been sporadic in various senses, and a description of very many ways by which in the modern clinic it is found to be imparted to the innocent.

Die Psychoneurosen und ihre seelische Behandlung, von PAUL DUBOIS. Uebersetzt von Dr. med. Ringier in Kirchdorf bei Bern. Vorrede von Dr. Déjerine. Zweite durchgesehene Auflage. Bern, A. Francke, 1910. pp. 484.

This book consists of a course of five and thirty lectures delivered in the University of Bern. The author is well known as being not

only a physician, but a psychologist and deserves to be reckoned as one of the pioneers of the new psychic treatment of disease. Some of the cases herein described are of great interest and, indeed, have become almost classical in clinical literature. Great stress is laid upon ethical dialectics, upon self-control, upon the doctrine that virtue is knowledge, upon moral insight, discipline, dialectics of pure feeling over against that of pure reason. The author freely grants that there are various forms of neurasthenia that cannot be healed by psychotherapy and upon which suggestion and belief, conviction, persuasion have little power.

Psychische Grenzzustände, von Dr. CARL PELMAN. Zweite durchgesehene Auflage. Friedrich Cohen, Bonn, 1910. pp. 316.

This is an interesting study of borderline or marginal phenomena, dealing with such very diverse themes as criminals, suicides, regicides, Cæsarian illusions, sex abnormalities, drunkards, bums, vagabonds, liars, querulants, victims of passion, pride, jealousy, peculiar people, fools, imperative ideas, hypnotism, illusions, ecstasies, seers and prophetesses, witches and possessed, psychic epidemics, etc.

Grundlagen für das Verständnis krankhafter Seelenzustände beim Kinde, von Dr. med. HERMANN. Hermann Beyer & Söhne, Langensalza, 1910. 180 p.

The author first treats of the brain, of concepts and thoughts and their disturbances; then of feeling, striving, willing, acting and their disturbances. This constitutes the general part. He then considers traits of degeneration in the different faculties, their most frequent types in children, congenital weak-mindedness and acquired disturbances, epilepsy, etc.

Wahnsinnige als Herrscher und Führer der Völker, von WILHELM HENCKEL. Otto Gmelin, München, 1910. 167 p.

These psychiatric historical studies are five in number: the first treats of Peter the third of Russia; the second of Paul the first of Russia; Nebuchadnezzar, the king of Babylon; Saul, king of Israel; and the last of Ludwig second, king of Bavaria.

Abnormal Psychology, by ISADOR H. CORIAT. Moffat, Yard & Co., New York, 1910. pp. 325.

The writer first explores the subconscious, defines it, gives modern theories about it, characterizes its mechanism and how it may become diseased. Then follow chapters on automatic writing and crystal gazing, testing and analyzing the emotions, sleep, dreams, hypnosis and the analysis of mental life with an interesting illustration. Under the diseases of the subconscious, he treats of losses of memory and their restorations, illusions, splitting of personality, hysteria, psychasthenia, neurasthenia, psycho-epileptic attacks. The book is an interesting contribution to the subject that is now greatly engaging the attention of psychologists the world over.

Anti-Pragmatism, by ALBERT SCHINZ. Small, Maynard & Co., Boston, 1909. 317 p.

This is a translation, with some modifications, of the author's book on the same subject in French. It is, on the whole, the ablest and most convincing criticism of pragmatism to be found in English. In an appendix the author answers some of the criticisms directed against the French edition of his work.

The Principles of Pragmatism, by H. HEATH BAWDEN. Houghton, Mifflin Co., Boston, 1910. 364 p.

The author here brings together many interesting essays of his which have appeared in various places before, and now gives them unity. He does not aim to construct a system but to show how in pragmatism we may establish the basal conceptions of a new philosophy of experience; hence his seven chapters are headed: Philosophy; Experience; Consciousness; Feeling, thinking, truth; Reality; Evolution and the absolute; Mind and matter.

The psychology of reasoning, by W. B. PILLSBURY. D. Appleton & Co., New York, 1910. pp. 306.

This volume is based on eight lectures delivered at Columbia University, in 1909. It attempts a brief statement of the place of logical processes, particularly judgment and inference in the concrete individual consciousness, not including the social aspects of reasoning, but its results in action for which the author refers to Baldwin and Dewey respectively. The chapters are the place of reasoning in psychology, belief, meaning and the concept, judgment, judgment and language, inference, proof and the syllogism, the nature of inductive proof, degrees of truth, modality and probability.

The influence of Darwin on philosophy and other essays in contemporary thought, by JOHN DEWEY. Henry Holt & Co., New York, 1910. pp. 304.

Besides the introductory lecture of 19 pages which gives its title to the book, the following articles are here printed, or reprinted from other publications: Nature and its Code, a conversation; Intelligence and Morals; the Experimental Theory of Knowledge; the Intellectual Criterion for Truth; a Short Catechism Concerning Truth; Beliefs and Existence; Experience and Objective Idealism; the Postulate of Empiricism; "Consciousness" and Experience; the Significance of the Problem of Knowledge.

Das Wesen des menschlichen Vorstandes und Bewusstseins: nach monistischer und dualistischer Auffassung. Von ALBRECHT RAU. Ernst Reinhardt, München, 1910. pp. 236.

This sums up this very ingenious biologist's theories upon the subject of the nature of consciousness with reference to monism and dualism. It is hard to state concisely his general theory. He holds that the torch of knowledge never goes out so long as there is any preserver or bearer for it and that it is the highest function of the state to provide and to keep investigation absolutely free. Again, all pure psychic reciprocities take place within the single soul and all intercourse of souls one with another is only indirect and mediated in physical ways. What we want is a test of reason by experience and history and to do this is the philosophic problem of the present day. Sense knowledge is the typical form of all kinds of knowledge.

Stoic and Epicurean, by R. D. HICKS. Charles Scribner's Sons, New York, 1910. pp. 412. (Epochs of Philosophy.)

It was a happy thought of this writer to bring the systems of Zeno and Epicurus together to show their fundamental similarity. Both exalted practice above theory and gave to sense and experience their full weight. Both were crude forms of realism such as the temper of the age in its reactions from extreme intellectualism favored. The success of the schools, too, was important, reaching its height among the practical Romans. The author has studied the original authors as well as the various recent writers who have gone into the field.

He treats of the earlier Stoics and pantheism; Stoic philosophy and epistemology; Moral idealism; the Teaching of the later Stoics; Epicurus and hedonism; the Atomic theory; the Epicurean theology; Scepticism in the academy; Carneades; Eclecticism; Ænesidemus and the revival of Pyrrhonism.

The philosophy of enlightenment, by JOHN GRIER HIBBEN. Charles Scribner's Sons, New York, 1910. pp. 311. (Epochs of Philosophy.)

After characterizing the age of enlightenment in general, the author begins the serious work of his book by considering Locke's inner and outer world; Berkeley's idealism; Hume's scepticism; the Materialistic movement in England; Rousseau's philosophy of feeling; the Philosophy of Leibnitz; the Conflict of typical philosophical influences in Germany; the Critical philosophy of Kant; the Practical influences of enlightenment.

The Place of Animals in Human Thought, by The Countess EVELYN MARTINENGO CESARESCO. T. Fisher Unwin, London, 1909. 376 p.

This book owes its origin to the question "whether the psychology of animals has not equally some relation to the science of religions?"—a problem raised by Count Goblet d'Alviella at the Oxford Congress in 1908. The topics treated are: Soul-wandering as it concerns animals, the Greek conception of animals, Animals at Rome, Plutarch the humane, Man and his brother, the Faith of Iran, Zoroastrian zoölogy, a Religion of Ruth, Lines from the Adi Granth, the Hebrew conception of animals, "A people like unto you," the Friend of the creature, Versipelles, the Horse as hero, Animals in Eastern fiction, the Growth of modern ideas about animals. The book is copiously illustrated with photographs from ancient monuments and other animal monuments.

Physiology of the Special Senses, by M. GREENWOOD, JR. Longmans, Green & Co., New York, n. d. 239 p.

This is designed for two classes of readers: students of psychology who wish to know more about the physiological side, who are advised to read in connection C. S. Myers's text-book of experimental psychology, and, second, to the physiologist. The book seeks to fill the gap between the ordinary text-book of psychology and original memoirs. The topics treated are: Fechner's law; Cutaneous sensations generally; Pain; Protopathic and epicutic sensibility; Taste and smell; the Sense of position and movement; Hearing, with historical sketch; the Ear; Comparative anatomy of vision; Retinal processes; Visual adaptation; Theories, trichromatic vision; Dichromatic vision; Historical theories; the Young-Helmholtz theory and hearing; Contrast space. There are 28 figures in the text.

✓ *The hygiene of the soul, a memoir of a physician and philosopher*. By GUSTAV POLLAK. Dodd, Mead & Co., New York, 1910. pp. 209.

This is a memoir of Feuchtersleben with special reference to his dietetics of the soul first published in 1838 and translated into English by Ouvry in 1852. Feuchtersleben was born in 1806 and died in 1849. Although his chief interest was medicine, he studied philosophy, literature and was a poet of note. The revived interest in psychotherapy makes this work a timely one.

Die mnemischen Empfindungen in ihren Beziehungen zu den Original-empfindungen, von RICHARD SEMON. Erste Fortsetzung der "Mneme." Wilhelm Engelmann, Leipzig, 1909. pp. 392.

This work is in partial fulfillment of the promise of the author in his first book entitled "Die Mneme"—the preservation principle in

the change of organic processes. He originally intended in this first continuation of his theme to attend solely to the pathology of the *mneme*, but has here radically modified his plans. After a discussion of terminology and the limitations of his scheme, he discusses in the first part the original sensations, their synchronous phase, single sensations and their associations, defines the idea of the field of sensation, discussing homophony, the *akolunthan* phase of original sensations. The second part is devoted to *mnemic* sensations and describes first the vanishing of the original and the persistence of the engramms. He then discusses the single engramms and their simultaneous complications, the individually acquired treasury of engramms, *ecphoria* and the various forms of association, components and indiscernibles, the relation of the *mnemic* to original sensations and their lapse, repetitions and the conditions of *mnemic* homophony, abstractions, differentials, modifications, comparisons, the engraphic action of homophonous components, contrast of original and *mnemic* sensations, general results in the sense of simplification and analysis.

Die Insekten in Sage, Sitte und Literatur, von KARL KNOREZ. Annaberg, Sachsen, Grafers Verlag (Richard Liesche), 1910. 151 p.

This is divided into four parts, the bee and honey, the flea and louse, flies and spiders, and all kinds of creepers and fliers.

L'Illusion Paradoxe et le Seuil de Weber, par MARCEL FOUCAULT. Coulet et Fils, Montpellier, 1910. 211 p. (Travaux et Mémoires de Montpellier. Série Littéraire V.)

The first chapter gives a brief history with preliminary experiments, methods and analysis. The second treats of measurements of the threshold, and the third of its conditions.

Klinische Beitrag zur Frage der Alkoholpsychosen, von WILHELM STÖCKER. Gustav Fischer, Jena, 1910. 289 p.

This work treats of the following classes of cases with the topics, Epilepsy as the basis of chronic alcoholism; Maniacal-depressive insanity; Dementia *præcox*; Other psychoses and psychopathies; and Obscure cases. Under these five groups, the author brings together descriptions of many cases.

Naturvölker und Kulturvölker. Ein Beitrag zur Socialpsychologie, von ALFRED VIERKANDT. Leipzig, Duncker & Humblot, 1896. 497 p.

This contribution to social psychology is an attempt to contrast the psychic and social life of primitive people with that of civilized and cultured races.

CORRECTION.

In a brief notice of Dr. Otto Lipmann's *Grundriss der Psychologie für Juristen* in the January number of the *Journal*, p. 174, it was inadvertently stated that the fifth chapter of the work dealt with the *Tatbestandsdiagnostik* "from the standpoint of Freud." This statement was unfortunate, as Dr. Lipmann is not at all a pupil of Freud, but on the contrary takes a critical attitude toward "psychoanalysis." Freud and his school make characteristic use of the *Tatbestandsdiagnostik* in seeking to discover psychic complexes *unknown* to the experimenter at the time of the experiment; the workers in legal psychology use it simply to determine whether a complex *known* to the experimenter is also known to the subject of the test.

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VOLUNTARY MOVEMENT

By E. C. ROWE

This paper aims to describe the cognitive aspect of the psychic experience in voluntary movement. It is therefore limited in two respects: first, to the sensory and ideational processes without attempting a detailed account of the feeling processes in such movements; second, it is limited to controlled *movement* and does not attempt an account of what may be called 'inner volition.' More specifically it is limited to the following four lines of discussion.

First. It aims to summarize critically the experimental and more important studies of voluntary movement, particularly such studies as bear upon the problem of the function of the "resident" and "remote" processes.

Second. To describe certain experiments undertaken for the purpose of a fuller description of the cognitive processes involved in voluntary movement and especially for the purpose of describing the gradual automatization of a highly volitional series of acts.

Third. To point out the bearing of certain anatomical, physiological and pathological data upon the rôle of (first) the centripetal impulse and (second) the sensation in voluntary movement.

Fourth. To point out the bearing of all these data upon the general theory of voluntary movement.

I. Literary Orientation

A cursory examination of current psychological text-books and literature reveals fundamental differences of opinion with reference to the universality of the image in mental processes.

Quotations from the following authors will suffice to justify this statement. In his recent text-book (p. 210) Angell writes: "The content of our thought is, so far at least as concerns the knowledge process, always made up of imagery." This position may perhaps be taken as the generally accepted view. Some writers, however, like Dewey hold that "every mental state is as *an existence*, an image." (Psychology, p. 204.) While not expressly stating such a view, Titchener at least implies in his Outlines of Psychology that an image peripherally or centrally aroused conditions every state of consciousness. Opposed to this view there is a small but vigorous group of writers whose position is well represented by Stout, Woodworth and Bühler. Stout argues, in his Analytic Psychology (Vol. I, p. 80), that imagery does not accompany the use of words in ordinary discourse. His reasons are that inasmuch as introspection fails to detect such images there should be "strong positive ground for assuming their presence." In the Bericht über den II Kongress für experimentelle Psychologie (p. 264), Bühler states his position thus: "Welches sind die Bestandstücke der Verschiedenen Denkerlebnisse? Antwort: Vorstellungen aller Art, aller Sinnesgebiete, Sach- und Wortvorstellungen, aber ausser ihnen viel häufiger, reicher und mannigfaltiger andere Gebilde, die am häufigsten als Gedanken, oder in Anlehnung an Marbe und Ach, als Bewusstseinslagen oder Ueberzeugung bezeichnet wurden. Es zeigt sich, dass man Vorstellungen und Gedanken im allgemeinen ganz sicher auseinander zu halten vermag."

After quoting from his own introspections and from those of others under experimental conditions, reporting an absence of imagery, Woodworth goes on to say (*J. Phil., Psy. and Sc. Meth.*, Vol. 3, p. 705) that it is necessary to assume the existence of conscious elements not reducible to sensory terms and that these elements must be looked upon not as "syntheses of sensory qualities, but simply and purely the qualities of particular thoughts."

It will be seen from the above that with reference to the existence of the image in the various mental processes psychologists are divided into at least three camps. (1) A group of writers holding that absolutely every state of consciousness implies the presence of imagery; (2) Another group insisting that at least all *cognitive* processes demand the presence of sensory imagery; (3) A group boldly declaring that even the cognitive processes are or may be carried on without the presence of a sensory or ideational image for every thought item in the process.

What now is the status of opinion when one narrows the field of inquiry to the mental processes that condition voluntary

movement? Here too there are 'camps' though the vagueness in the use of the term 'voluntary,' coupled with its peculiar individual uses and the various interpretations of the current kinæsthetic theory of voluntary movement, make it much more difficult, if not practically impossible, to classify the different positions found in the literature upon the subject. James' exposition (Prin. of Psy. Vol. II, pp. 486-592) of the kinæsthetic theory reveals, as Woodworth has pointed out, the possibility of a wider and a narrower interpretation of this theory. The theory is so familiar that a summary of it seems unnecessary.

The latitude of interpretation which James' statement permits reflects what one actually finds in both earlier and later writers, namely, in some cases, a limiting of the mental antecedents necessary to a voluntary movement to "resident" processes with the possible inclusion of remote visual effects. Ziehen (Intro. to the Study of Phys. Psy., pp. 246-248) and McDougall (Phys. Psy., pp. 163-165) are clearly inclined to this narrower view. On the other hand Münsterberg (Willenshandlung, pp. 88-96) and Angell (Psy., pp. 404-409) give the theory a broader interpretation so as to accord a fuller recognition to James' "very remote" sensible effects.

One of the earliest criticisms, based upon experimentation and observation, of this entire position is found in an article by Kirkpatrick (The Development of Voluntary Movement, *Psy. Rev.*, Vol. 6, 1899, pp. 273-281) in which he reports a study of the motor development of one of his own children. His observations lead him to the conclusion that "there is no evidence that his (the child's) consciousness is concerned at all with the movements he is making in order to get hold of the object and bring it to him,—attention to the movement itself hinders rather than helps in learning the movement." (p. 280.)

Another investigation having an important bearing upon this problem is Bair's study of how we get voluntary control of an entirely unused muscle, the *retrahens aurem* by means of which the ear is moved. (Development of Voluntary Control, *Psy. Rev.*, Vol. 8, 1901, pp. 474-510.) Twelve of the fourteen persons who acted as his subjects had absolutely no control of the ear at the beginning of experimentation. In order to give his subjects the kinæsthetic sensations and images produced by the movement of this muscle, Bair at first caused the muscle to contract by means of electrical stimulation.

His results may be stated both negatively and positively. Negatively he found that the kinæsthetic sensation or image of the movement of this muscle was not in itself sufficient to bring about voluntary control. Positively, he found that before control of a muscle is established the sensation produced by the

reflex or involuntary movement of that muscle must be experienced in association with the sensations from the near-by muscles over which voluntary control has already been gained. He says: "The contraction could not be made voluntarily, not even after it was repeated (artificially of course) a sufficient number of times thoroughly to impress the sensations and definitely to fix the association between the muscle sensation and the visual impression of it." (pp. 499-500.) This appears to imply, that both "resident" and "remote" (visual) sensation were given the reagent without his being able to make the movement voluntarily.

Bair explains that getting control of such a muscle means three things: (1) Producing the movement accidentally through the spread of excessive energy into outlying muscles not yet under voluntary control thus giving a new muscle sensation, or sensation from a muscle from which sensations have not hitherto been experienced. (2) It means the association of this "new" sensation with sensations from muscles already under control. (3) The dissociation of this sensation from the complex of sensations in which it originally arose, so as to be caught separately by attention.

If one distinguishes between sensation and image, a distinction which certainly is not one merely of words, then the findings of Bair's study do not lend unqualified support to the kinæsthetic interpretation of voluntary movement, which explains the whole process in terms of memories or images, "resident" or "remote," of movements which took place involuntarily, or at least non-voluntarily. Bair shows that the memory, or image, of how it feels or looks to have the ear moved is not sufficient to bring about its voluntary movement.

A third experimental study with important bearings upon this question is Woodworth's investigation of "The Cause of a Voluntary Movement." (Studies in Philosophy and Psychology, Garman Memorial Volume, pp. 351-392.) Woodworth's subjects were required to make various movements, some of which, as Angell points out, were probably "too well mastered and too habitual to throw fairly into the foreground the sensory-ideational elements emphasized in gaining control of them." ("Studies in Psy., *Journal of Phil., Psy., and Sci. Methods*, Vol. 3. p. 241.) It is, however, to be noticed that Angell's use of the term "sensory-ideational" implies a failure to distinguish between sensation and image, a distinction of which Woodworth makes use and which has important bearings upon the whole problem.

Out of 128 single introspections Woodworth summarizes the imagery as follows: 27 reported kinæsthetic, 27 reported

visual, 17 reported imagery of other kinds, 30 reported only peripheral sensations, 27 reported an absence from the field of attention of all sensory or ideational imagery. (p. 361.)

Woodworth states that in many of the instances in which imagery was reported, there was what he and Bühler call an "inadequacy" of imagery. This was particularly true of kinæsthetic images which, when present, were found to be unlike the movement that followed. "Sometimes the kinæsthetic represented movement was much briefer than the real movement. Sometimes the kinæsthetic image pictures a slow movement when the resulting movement is rapid. In general this sort of image frequently contrasts with the resulting sensations of actual movement. If we picture how a movement is going to feel and then make the movement we find it feels very different from what we anticipated." (pp. 362-363.)

This illustrates what these authors mean by the "inadequacy" of images. Bühler goes so far as to argue that the "Inadäquatheit zwischen dem Gedankengehalt und dem, was vorgestellt wird" (Kongress für experimentelle Psychologie, Bericht II, 1906, p. 264) is an indirect evidence that thought processes are carried on without imagery. Doubtless he would hold too, with Woodworth, that voluntary action may take place at the instigation of a "naked," imageless, thought.

This concept of the adequacy or inadequacy of mental imagery appears to be borrowed from the physical sciences and transferred bodily to the field of psychology. The argument seems to run thus: If a kinæsthetic image is *the* cause of a voluntary movement, then as such it must contain as much as is contained in the result, just as in physics the cause must, in terms of energy, be equivalent to the effect. Against this position the following arguments may be urged:

(1) The concept of "Adequacy" in the sense that, to be the cause of a movement, the mental image must contain as much as will be contained in the movement, reduces itself, when logically carried out, to a *reductio ad absurdum*. Bühler applies this concept to the thinking process, as has been shown, and concludes that images are not necessary to complex thinking processes because the image is frequently "inadequate." But why stop with movement and thinking? Why should not the same argument be applied to all mental processes and finding that everywhere images are "inadequate," as they certainly are, in the sense in which these authors seem to use the term, conclude that images are not essential to any of our mental processes, and thus rule the image out entirely and regard it as a really useless thing in all forms of experience?

(2) It is not always true that "the adequacy or inadequacy of images is a point of importance in judging how much of a real causal function the image has in the production of movement." (p. 362.) Many things come in to modify this "importance" of the adequacy or inadequacy. Reflexes, instincts, habits and "psycho-motor" tendencies all come in to supplement any lack that may exist on the part of the initial image. And, what is still more to the point, the immediately inflowing sensations, once a movement is started, are of incalculable service in the further control of it. Woodworth himself has pointed out that the coördinations of a movement are unconscious processes. The details, therefore, of a movement need not be included in the image in order that it be adequate.

(3) This idea is not a psychological concept. It is rather a logical, *a priori* idea, and as such practically the same argument may be urged against it that Woodworth presents in opposition to the image theory, namely, that it is too "schematic." As a sort of logical necessity it is apparently *assumed* that *to be adequate* the image *must* contain what will be contained in the forthcoming movement, and *then*, when introspection reveals that the image does not contain qualitatively and quantitatively what the movement sensations reveal, it is *therefore* declared "inadequate."

Woodworth refers also to his own attempt to learn to move the great toe in isolation from the others. "The establishment of complete voluntary control is a very gradual process." (p. 36.) He finds, too, with Bair, that the first success comes by accident. Gain in control is then made by directing attention to the member itself. In this, too, he agrees essentially with Bair who found that the final stage in getting control of a muscle consists, as has been stated, in dissociating the sensation of the movement of the muscles in question from the sensations derived from other and already controlled muscles. But while the kinæsthetic *image*, and in fact imagery of every sort, is entirely unnecessary, Woodworth holds, that kinæsthetic *sensations* from a member are indispensable, maintaining that "sensations are indeed always present, as contributing factors in determining the act. They represent the existing situation with reference to which the act is performed, and the act is determined by the existing situation as well as by the intention." (p. 383.) This position will be referred to again in the discussion of the relation of sensation to movement.

Thorndike calls attention to five facts, which in his judgment argue against the kinæsthetic theory.

(1) The same imagery will frequently be found employed

in willing *not* to do a thing that we find in willing to do it. This fact he regards as a good reason for the suspicion that in willing to do a thing the image of doing it comes as a natural consequence of the idea and *not* "as a necessary dynamic factor in the action."

(2) "We can will acts images of whose resident sensations are not obtainable." This position, he holds, is supported by the fact that we can perform many operations in less time than it takes to call up the imagery. For example, one can write a combined series of figures, letters, dots and dashes in less time than it takes to call up the various resident or remote images representing the various movements. To this, doubtless, the supporters of the image theory would reply that all such movements have been practiced until so completely habitual as no longer to require the presence of the imagery that functioned at the time the movements were learned.

(3) In many of our voluntary acts we will to do things so complex that it would take all of us several minutes to call up the imagery of the series of movements involved. For example, we can will to draw a polyhedron of 28,000 sides.

This objection is apparently based upon the erroneous idea that such a complex act as the example given is a single act. To draw a polyhedron of 28,000 sides is clearly not a single act but a connected series of acts, from both psychological and physiological points of view; and therefore, if it calls for imagery at all, practical necessity would demand a series of images and not a highly complex image before beginning the drawing.

(4) "In trying to get any one to make a voluntary movement we rarely take means specially useful in calling up the images of resident or remote sensations, and often do take means specially to prevent their appearance."

(5) "If we insist on the image's effective presence we make voluntary action sharply discontinuous with involuntary action." (*The Mental Antecedents of Voluntary Movements, Jour. of Phil., Psy., and Sci. Methods*, Vol. 4, 1907. pp. 40-42.)

The experimental study by Ach, *Ueber die Willenstätigkeit und das Denken* (Göttigen, 1905,) is perhaps the most systematic investigation of this question thus far made. Ach's position, it will be observed, agrees in a measure with the position of Woodworth and Thorndike.

Previous studies of voluntary movement, Ach finds, have for the most part been confined to reaction-times and have, therefore, neglected the psychological processes involved. The analysis of these processes is the author's object. He used simple and complex reactions with the Hipp chronoscope as the recording instrument.

With relation to the content of consciousness present in the various reactions employed Ach expressly states or implies the following observations:

(1) The particular content of consciousness present in a given reaction varies, first with the individual tendencies. ("Individuelle Veranlagung spielt hier eine grosse Rolle.") It varies also with practice and, thirdly, with the apperception of the stimulus.

(2) The mental processes involved in all voluntary action are in no small degree determined by what the author calls "determining tendencies." These tendencies are the results of the presence of an idea of a *goal* or *end* to be accomplished by the reaction. In themselves these tendencies may be entirely unconscious, but they are nevertheless essentially effective in shaping the mental content controlling the reaction.

(3) When the reaction itself takes place there is no discoverable "will" process or content, as such, present. All this comes at an earlier period when the subject consents, as it were, to the procedure as a whole.

(4) Ach and his subjects discover in the content of the volitional consciousness what may be classed as two sorts of sensation, both, however, being *necessarily* present *only* in the early and unlearned stages of the reaction. Both may be entirely absent at the later and familiar stages. The first sort of these sense processes is made up of the usual peripheral sensations set up by the stimulus. The other class, highly important in Ach's account, is essentially kinaesthetic sensations. Ach prefers to call them "intentionalen Bewegungsempfindungen." They are, however, he tells us, "Spannungsempfindungen" and have their origin in the muscles. Their function is to give us the trend or direction ("Richtung") of the forthcoming movement. Ach speaks also of sensations which are mere accompaniments—Begleiterscheinungen—which do not appear to serve any particular function.

(4) Then there are all sorts of memory images, visual, acoustic, kinaesthetic, etc., which present themselves most frequently in the early stages of voluntary action and then disappear more or less. Upon the presence of these images, which constitute the essential elements in the image theory, Ach does not appear to lay much stress.

(5) Finally there is present, particularly after practice and frequently in the period after the ready signal has been given (Vorperiode), a functional mental content—and sometimes it constitutes the entire consciousness of the moment—in which no *imagery at all* is to be found. This consciousness is at times entirely without sensations of any kind; even the highly important "intentionalen Bewegungsempfindungen" are wanting.

"Wir bezeichnen dieses Gegenwartigsein eines unanschaulich gegebenen Wissens als Bewusstheit." (p. 210.) This "Bewusstheit" which is characterized by the "Wissen" that it contains cannot be further analyzed. However, in his discussion of this imageless and sensationless state of consciousness Ach brings out the following facts concerning it: (a) Its constituent parts (*Bestandteile*) fade with repetition. (b) The intensity of the *Bewusstheit* decreases with practice and familiarity. (c) The whole process may be designated as a lessening of concentration of attention.

(6) The further fact is to be noticed that the *Bewusstheit* is *always* immediately preceded or accompanied by sensations (*Spannungsempfindungen*), visual, acoustic, kinæsthetic, etc., or by a memory image of such sensations. The clear, unequivocal content of the imageless consciousness does not, however, in Ach's judgment warrant describing it in terms of indistinct sensation or memory representations.

(7) It would appear then that this imageless content is not a *derived* consciousness but is, as Woodworth puts it, an original consciousness having a distinct quality of its own.

Another study throwing light upon the problem of the necessary elements functional in voluntary movement is that by Downey. (*Controlled Processes in modified Handwriting. Psy. Rev. Monographs*, Vol. 9, No. 1, pp. 1-148.) Downey studied handwriting under modified conditions, such as elimination of visual control by blind-folding, mirror writing, inverted writing, embarrassment of motor control with and without vision, etc.,—and also under conditions of distraction. The more important results bearing upon this study may be summarized as follows:

(1) Throughout the tests the reagents fell into two groups, one emphasizing visual, the other "grapho-motor" control.

(2) "Individual variation was the extent to which the subject had recourse to grapho-motor control either conscious or automatic." (pp. 51-52.) But "when the break up in the motor coördination was sufficient to demand the acquiring of a new writing reaction the former reagents (grapho-motor group) tended to use consciously more motor material than did the latter (visual group). The form of the break-up usually occasioned intensification of the visual control." (p. 140.)

(3) In the distraction tests the grapho-motor group "showed the strongest tendency to write more or less automatically; the second group the strongest tendency to control writing consciously." (pp. 125-126.)

(4) "Mirror writing increased the tendency to rely on grapho-motor control, a surrender partly to motor automatism but partly also a control by motor anticipation," but "in in-

verted writing . . . there was a gain of visual over motor control." (p. 52.) In the case of one reagent "the visual material represented a pseudo-control, but with the embarrassment of the motor situation it became actually directive." (p. 46.)

(5) A negative result of much importance for a later section of this paper is to the effect that "no evidence was found for the initiation of a *voluntary* act [*italics* Downey's] of writing without a sensory cue of some sort." (p. 142.)

(6) Another important result is that "throughout the whole series of experiments the *report* coming from the writing in terms either of kinæsthetic or visual sensations and images, proved to have a highly important function as part of the writing cue." (p. 142.) "A sensory kinæsthetic report on the movement as it proceeds is of course usually present, although it varies apparently in vividness and accuracy from individual to individual and has a different value for the same individual under different conditions." (p. 7.) "Many lapses occurred in B's writing of the verse, due, he asserts, to the lapsing or retarding of the grapho-motor report." (p. 63.) "Open errors frequently resulted from the lapsing of the grapho-motor report from the writing and were most often found in reagents of the first (motor) group." (p. 142.)

(7) "When automatic writing occurred, it was apparently, purely physiological in character." (p. 142.)

(8) In contrast to Ach and Woodworth's findings, Downey brings forth no evidence whatever in favor of an imageless consciousness which is at the same time operative in effecting control.

(9) Although "the experiments offer some evidence (which, however, is not 'unambiguous') for the existence of grapho-motor imagery"¹ (p. 142), it is, however, perfectly clear that so far as control is concerned there is no evidence showing that such imagery is necessary.

To turn now to the broader aspect of this subject, it would appear that there are three important psychological concepts that have an indirect bearing upon the problem of voluntary movement. They are: (1) The Reflex arc concept. (2) The concept that *all* consciousness is motor. (3) The concept of motor and sensory forms of reaction.

Dewey has shown (The Reflex Arc in Psychology, *Psy. Rev.*, Vol. 2, 1896, pp. 357-370) that the reflex arc concept should be given a fuller recognition in the study of movement and also in the study of all conscious processes and that once given such a recognition this concept necessitates a readjustment of psy-

¹Cf. Book: The Psy. of Skill, p. 54. Montana University Studies, Vol. I, No. 1.

chological procedure. Our present terminology and analyses, he points out involve an unfortunate dualism, like the dualism of body and soul to which it harks back. We have been accustomed to think of the sensory stimulus as one thing, the central activity, standing for the idea, as another, and the motor discharge, standing for the act proper, as a third. But instead of viewing these elements as each a distinct entity, they should be viewed as "divisions of labor, functioning factors, within the single concrete whole, now designated the reflex arc." (p. 358.) A stimulus always breaks into a coördination and not into a sensation mass. Whenever we turn round upon ourselves what we find is a coördination, an act taking place. Within this act lie both sensation and movement as phases of it.

On the basis of this conception this much may be laid down, namely, that voluntary movement never starts *de novo* and never begins with a mere sensation mass; it always breaks in upon an existing coördination. But when consciousness plays a part in the formation of a new adjustment or rather the modification of an existing one, attention is directed to the sensation phase of the coördination because this phase represents the existing situation with reference to which the adjustment is made. When, now, one asks, precisely what elements of the sensation phase of coördination must attention be directed to, any of it, all of it, or only *certain* elements of it, the position of the image theory at once becomes arbitrary; for, to be consistent, it must hold that *immediate sensations are not* a sufficient basis upon which to make a volitional movement,—there must be present *images* of sensations experienced in involuntary and reflex activity. The objections here raised are that this position is first of all highly arbitrary, and secondly, as Thorndike has pointed out, puts volitional over against involuntary activity so as to make the former radically distinct from the latter. Moreover, whether we regard voluntary movement as an activity developed out of original reflexes, or whether we regard reflexes as the products of conscious impulses and volitional control the relation between them, in either case, is rendered arbitrary if we take the position that in voluntary action *certain* senses and ideational elements are the essentials while in other forms of activity any peripheral or central processes that bear a relation to the situation with reference to which the act is performed are sufficient.

At this point the theoretical exposition of "Consciousness and Movement," by Judd, based upon a variety of experimental studies by himself and his associates in the Yale Laboratory, is significant. Judd calls attention to the distribution of the sensory areas in the cortex. "The sensory centres," he says, "except the centre for touch, are distributed in the

highest brains in such a way as to be relatively isolated from each other and from the motor area. The association areas which link these widely separated sensory centres are clustered in unmistakable fashion around the motor area." (*Psy. Rev. Mon. Supp.* 7, 1905, "Movement and Consciousness," p. 207.) Judd holds that all paths lead to the motor area through the associational tracts; that the higher senses do not "reach the motor areas *through* tactual centres." (p. 209.) He points out that muscular and tactual sensations are unrefined and that "requirement of sensory control and refinement of movement go hand in hand. Why, then, should the conscious processes in which visual factors are fused be continually referred back to a primitive form of sensation for their explanation?" (p. 218.)

The bearing of Judd's contention upon the problem of this paper is evident. All impulses move toward the motor area; sensations do not arouse other sensations but motor responses. The higher and phyletically later sense processes are more "refined" than the older tactile sensations. The later acquisitions, therefore, more accurately represent the situations to which adjustments are made. There is, therefore, no reason why images of these later and more refined sense acquisitions should not, so far as any imagery at all may be concerned, play as original and important a rôle in volitional acts as any other imagery.

In the language of James, "All consciousness is motor," a fact so frequently demonstrated as to be a commonplace. But if the image theory of voluntary action is strictly interpreted some elements of consciousness under volitional conditions require the presence of additional conscious data in order to become effectually motor. To put it in the physiological terms in which Judd has expressed the situation, some sensory impulses cannot pass over into motor impulses without first arousing certain other sensory activities which are the physiological correlates of previous sensory experiences obtained under involuntary conditions. In fine, if the image theory is interpreted in accordance with the scientific usage of psychological terms it involves a serious modification of the proposition that 'all consciousness is motor' in case of all reactions above the level of the involuntary and the habitual. Here once more we are face to face with a break between the voluntary and involuntary which in turn involves a gap in the image theory not yet bridged over by its supporters.

There is a third concept current in the psychology of movement which is apparently difficult to explain on the basis of the image theory. This is the notion of sensory and motor forms of reaction. When the subject is making reactions that

are highly volitional, as has been frequently the case in studies of reaction time, how is it possible to have a sensory type of reaction at all? Suppose, for example, the subject is reacting with his toe or his lips as in the case of Angell and Moore's study. In this case the image theory would demand that before the subject could make his reaction there must appear in consciousness some kind of image of the movement itself. Here, then, it would be misleading to speak of a sensory type of reaction for the reason that the image of the stimulus would have to be followed by an image of the movement either as felt, seen, or experienced in some other form, before the movement could be made. That the intervention of such an image does not necessarily take place, particularly after practice, is pretty well established by Ach and (by implication) by Downey and by experiments to be described later in this paper. This makes the concept of sensory and motor types of reaction correspond to what the names imply and to what has been commonly understood to be the distinction between them.

In interpreting his own theory James has said: "In the chapter on the Will we shall learn that movements themselves are results of images coming before the mind, images sometimes of feelings in the moving part, sometimes of the movements' effects on the eye and ear and sometimes (if the movement be originally reflex or instinctive) of its natural stimulus or exciting cause." (Vol. I, p. 445.) The literature reviewed and the arguments already set forth demand a modification of this position.

It is clear that much of the older literature bearing upon voluntary movement is not only general in character but to a degree at least *a priori* in its derivation, and hence, as Woodworth has pointed out, logical and schematic rather than strictly psychological in its treatment. The weakness of these older studies is therefore primarily methodological but a weakness which in turn brought errors of result and interpretation. Another methodological error that has crept into certain more recent studies of voluntary movement is that of studying thoroughly practiced movements, and from such study drawing inferences with reference to voluntary movement in general. 'To instruct a person verbally to make certain very familiar movements and then from the absence of supplementary imagery to draw the inference that imagery in general is not necessary for voluntary movement' (*cf.* Angell's review of "The Cause of a Voluntary Movement." *Jour. of Phil. Psy. and Sci. Meth.*, Vol. 3, pp. 641 f.), is not only an error of fact, as will be shown later in this discussion, but is primarily an error of procedure which leads directly to the error of fact.

To avoid such errors the study, to be described, is based primarily upon new and unpracticed movements and also upon highly voluntary movements practiced until they became nearly automatic.

II. Description of Experiments

The movements studied were the following:

(1) Writing ten standard words on an apparatus resembling a typewriter until the reactions became practically automatic.

(2) Writing with the hand under novel conditions and also with distraction.

(1) Typewriting experiments.

The problem in these experiments was to describe not only in cross-section the cognitive elements involved in control but also to trace the gradual elimination of these conscious elements up to the point of automatism.¹

The apparatus used was constructed by fastening ten small rubber bulbs upon a board about sixteen inches long in such a way as to fit comfortably the fingers and thumb of each hand when placed upon the "keyboard." All the bulbs were connected by means of a rubber tube to a single tambour writing upon the drum of a Ludwig kymograph, which was placed on the opposite side of a screen from the subject. The bulbs were "lettered" by means of letters printed upon a cardboard strip and corresponded to the lower row of keys of the Blickensderfer typewriter. From these letters the following list of ten four-letter words was made up: *tons, hear, tide, road, hits, shin, nods, dear, heat, iron*. It will be observed that out of a total of 40 letters each letter occurs four times, thus giving all the fingers equal practice. A graphic record of the writing of these ten words was taken on a kymograph drum at the beginning of each sitting, the writing being to dictation of the words by the conductor of the experiment. This was followed by a brief "practice" period, at the conclusion of which a second record was taken in the same manner. At first the "practice" consisted of ten repetitions of the standard words with the list placed where it could be conveniently seen; but after the list had been memorized the words were written also five times with eyes closed in addition to the ten times with eyes open.

The difference between these movements and those of gen-

¹The experiments of this section were undertaken before the publication of Book's study above mentioned, and with only indefinite knowledge of his general results. Since then his paper has been read with care and in many ways found to support the main theses of this paper. A review of it has seemed impractical, however, on account of limited space.

eral typewriting consists in the fact that after the subject had once learned the order of the letters on the keyboard and the list of words to be written he knew at each point in the series of movements just what was to be done next. Indeed, in their general nature these movements resemble complex reaction-time experiments involving discrimination and choice, rather than ordinary typewriting, differing from the former only in the fact that the stimuli were given in a uniform order.

The four subjects *C*, *D*, *E* and *R* who served for this experiment were graduate students in the department of psychology of Clark University. Each had had experience as a reagent prior to this experiment. *C*, *D* and *E* gave thirty sittings, each writing the list on the average nearly 500 times. Besides the brief "practice" period mentioned, *R* also wrote the list from ten to twenty times per day for additional practice, thus writing the entire list a total of over a thousand times during a period of seven weeks.

That these movements became practically automatic with daily practice in a few weeks' time is not surprising when the simplicity of each movement taken by itself physiologically is considered. The simplicity of the movement as compared with ordinary writing or throwing a ball, is in itself sufficient to account for the rapidity of automatization. This fact was a decided advantage since it permitted a genetic study as well as a cross-section study which was all the experiments on ordinary writing afforded.

The most striking characteristic of the first stage of control in these type writing movements is the pronounced dependence upon the eye on the part of every subject. This was true not only when writing from the list in "practice" but also when writing to dictation for the records. In this first stage while writing to dictation the subject apperceived the word by pronouncing it in inner speech, then repeated, in inner speech, each letter while running his eyes over the "keyboard" to locate each letter of the word before the writing (*i. e.*, pressing the bulb) was begun. In addition to fixating each letter before the word was begun, it was again attentively fixated when the corresponding bulb was pressed, making a double visual sweep of the cardboard guide which carried the letters. Not only was this true, but in case of the two subjects *C* and *R* who had had no especial practice in controlling the fingers separately, such as is afforded by typewriting or piano-playing, *the attentive fixation was extended from the letters on the cardboard to the corresponding bulbs as each was pressed.* The other two subjects, *E* and *D*, one a practiced piano-player, the other a typewriter, gave no indication of fixating the bulbs during this first period.

This difference, we are inclined to believe is not accidental, but directly due to skill previously gained in the separate control of the fingers, the skill thus acquired showing itself in the ability to write as readily without bringing the point of regard so close to the fingers as was done by the other two subjects. *C* and *R* focussed *every separate objective* item involved in the writing with the exception of the fingers.

While inner speech was distinctly conscious at this stage, there are no reports showing that it became a focal element. It would seem that the inner speech in this case was very much the same as it is in silent reading; attention is upon the page and the meaning and never upon the inner speech as such. Inner speech reacts upon and modifies what is attentively seen (or heard) instead of what is seen (or heard) reacting upon and modifying what is present in inner speech.¹ Indeed the latter would involve a complete reversal of the situation as it is given to the subject, for the reason that what he starts with is, in practice, a fixated word, and in dictation a heard word. Meaning always implies the associative play of another process upon the one that *has* the meaning. It is clear that a sense process cannot possess *conscious* meaning in and of itself; it must call up something, be reacted upon by something, to have meaning. In this case the visual sensations flowing in from the list of words or the auditory sensations flowing in by virtue of the dictation, are reacted upon by inner speech and thus get their meaning. In this sense the "inwardly spelled word" is the immediate cue for the consequent movements, *although the entire cue is always the total situation as perceptually cognized by focal and marginal processes*. Inner speech was at first more or less strongly muscular on account of the effortful nature of the whole performance. Another characteristic of this first stage of the movements under discussion is that the muscular sensations, though clearly conscious, are never focal, except when by virtue of their intensity they may *distract* attention from the copy. For example, at his first sitting subject *C* complains of "a feeling of jerkiness" and that the "muscular currents are interrupted" and significantly adds, that the process is "not pleasant." This subject was the first to break away from the visual use of the "key-board" which he did at his fourth sitting, having learned in their order the letters on the key-board. Here then was an opportunity for attention to muscular sensations to show itself if occurring at all during this period of learning. But the report for this sitting contains the statement that the subject is "conscious of the bulb to be pressed rather than of the finger to do the pressing." It is

¹ Cf. Book: *op. cit.*, p. 157.

only when the situation is new and highly stimulating and the consequent "overflow" considerable that the muscular sensations claim direct attention, and perhaps only then when the subject is on the lookout for them. Here again we have an instance tending to show that when learning to make movements that involve objects other than the body attention is upon the sensations arising outside the body and not upon those arising within the body.

With minor exceptions there is no evidence that during this learning period our subjects made use of either resident or remote imagery of the forthcoming movement. There is not an introspection indicating that any one thought of how the movement, about to be made, was going to "feel" or look. The only functional imagery, distinguishable from the sense and perceptual processes involved, was inner speech, and even this was at this time more or less muscular. One of the "minor exceptions" just referred to, was the idea of the result of the movement, which figured only in the dictation writing. This idea was the idea or visual image of the result of the movement of the writing point upon the drum surface. It is therefore clear that during this first and most difficult stage of control, attention is objective and perceptual.

The second stage leading towards automatization is characterized by a "short circuiting" process in which certain perceptual elements drop out, and, if consciously present thereafter, are represented by memory images. The first elimination is to be found in the disappearance of excessive eye movements over the key-board when a general idea of where to look for the letters has been reached. At this stage the different methods presented by different subjects began to manifest themselves. As previously stated, *C* consciously freed himself from the key-board very early by learning the letters for each bulb and then using the "touch method." Even on the second day "the effort is to make automatic the coördination between *memory* impressions of the letter on the cardboard and the motor impulse." On the fourth day he is "trying to avoid looking at the cardboard entirely," but he has not yet "got the letter translated into the bulb," *i. e.*, he has not "short circuited" so completely as to "take directly from list to bulb." *C*'s short-circuiting method was therefore that of eliminating eye movements over the letters on the cardboard and substituting for them the visual memory image of each letter and its location until a direct association between letter and bulb was set up. *E*, trained upon the piano, followed essentially the same method. For example, at the fourth sitting *E* "can make the movement of pressing the bulbs in shorter time than it takes to place (*i. e.*, to fixate)

the letters on the cardboard," and is already visualizing the letters infrequently and remembers their places in terms of motor activity. Here we have first the elimination of direct vision of the letters and a little later the elimination of the visual memory images of the letters, at least to a degree.

R fell into the other method, that of learning the list of words early in the practice and thus freeing the eyes for constant use upon the key-board. This eliminated eye movement from list to key-board and *vice versa*. On the second day *R* speaks of a greater ease in execution in consequence of having now "a general idea of where to look for the letters." After a few sittings *R* still further eliminated direct eye-movements from letter to bulb, so that the pressing of the bulb followed immediately upon the fixation of the letter. *R* also reduced eye-movements by gradually ceasing to fixate each letter as the corresponding bulb was pressed, and instead fixated the central part of the cardboard with only slight fluctuations on either side.

D never made direct eye-movements from the letter on the cardboard to the corresponding bulb. His method was essentially the same as that of *R*, with the exception that he used the list of words for a longer time. He did, however, early begin to eliminate eye-movements over the cardboard and the fixation of each letter. At the second sitting he thinks there is some motor memory present and finds that when the movement is once started the "visual element becomes peripheral and almost drops out." At his eighth sitting he "fixates only the central part of the cardboard and makes use of the outlying letters in peripheral vision." A day or two later he reports that he "did not have to look at the letters on the cardboard." This was on the day when eye-closed "practice" was begun. He was, however, still looking at the list of words, which practice continued for a considerable time after the words had been committed to memory.

These details have been given to illustrate what is meant by what has been called the "short circuiting" process. They illustrate the general principle of initial excess of activity which characterizes all learning processes, a principle to which voluntary movement offers no exception; for voluntary movement is genetically a learning process, though it differs from certain learning processes in this respect, that much of what is eliminated is not necessarily faulty but at the beginning useful and even necessary. It is therefore not so much an elimination incident to "trial and error" as an elimination due to what may be called a knitting up of the associative links between the various sensory and perceptual processes involved in the control.

Another fact to be observed here is that, as this "short-circuiting" process takes place, control as a whole shifts from what at first was almost, if not wholly, peripheral, involving a hunting about with effort, to what may be called a central control, securing precision and ease in the sense that the operator knows just what to look for and what to do, and in the further sense that the association links between the factors involved in control function without mental effort. In figurative language the subject has at his command a panoramic map of the whole mental situation and the transition from one focal point to the next on this map tends to become automatic. As the "short-circuiting" and knitting-up process takes place, the tendency is clearly present to pass from what was first a disjointed mass of details to what becomes a complex unity. Gradually organization comes out of chaos; and, with organization, details as such lose their distinctness and merge into a situation which in course of time requires attention not to separate details or series of sensations and images, but only to a *total situation*. But the situation does not become an attended-to situation until the "short-circuiting" process has reached a certain degree of completeness. Reduction of sensory and perceptual data to a minimum is therefore necessary to bring about a movement-situation in which the details run off without attention to them as such.

The possibility of consciously organizing a given group of movements into a control situation requiring only a single "set" of attention is therefore determined by the nature of the movements on the one hand and the span of attention on the other. Just as some of our movements surpass the physiological limit of becoming reflex—at least in a single life-time—just so other of our movements surpass the conscious limit of becoming controlled by a single "set" of attention. This "set" of attention, *i. e.*, attention to the organized situation, is the third and last stage reached in the movements under discussion. A fuller description of this "set" as it appeared in the experience of the four subjects participating in this experiment is now in order.

It is scarcely necessary to call attention to the fact that this so-called "set" of consciousness is not a purely mental state but clearly a psycho-physical condition involving the whole musculature as well as the neural and conscious aspects of the organism. It is a movement-consciousness with habituated motor tendencies. But while the motor processes accompanying and flowing from the "set" become habitual and ultimately highly automatic, and while the distinguishable *contents* of the "set" are also highly involuntary, *the maintenance of the "set" itself is the remaining voluntary aspect of the whole process*. The introspections of both *E* and *R*, with whom the "set" was most

highly developed, abundantly illustrate this fact. For example when *E*'s attention, during the last days of experimentation, wandered too far away from the process the writing stopped. The inability to continue the writing was not, however, due to "any less attention to the details" but to "lack of attention to the general situation." *R* reports that all that is required to keep up the proper movements is a certain general "direction of attention" or "general attitude," and that if this is maintained the details take care of themselves.

But if the details as such receive no attention how is it that mistakes are recognized immediately after they occur, a fact which was often noticed? If a wrong movement is recognized, it must rise to the level of consciousness as a distinct movement, and this is precisely what happens. When the movements run off correctly and smoothly we have a condition which Hobhouse well describes when he says that the "crude sensation has assimilated certain characteristics which, if disentangled, form the contents of ideas, but which are not disentangled as long as they are assimilated. Prominent among these are motor-impulses. We may call them acquired sense impulses, and say that the present stage of assimilated experience postulates sensations and feelings as its data and produces acquired sense impulses passing into habit as its result." (Mind in Evolution, p. 101.) But the sensations from wrong movements have not acquired the idea 'characters' and the 'motor impulses' that characterize the sensations from correct movements and hence at once rise to the level of distinct consciousness. They are instantly disentangled from the other contents of the "set," for they never really belonged to the "set," and being alien to it, at once attract attention when they appear. It is therefore clear that the muscular sensation or after-image of the correct movement is quite a different thing from the muscular sensation or after-image of the wrong movement. The former is reacted to as a sensation in peripheral consciousness, if conscious at all, the latter gives rise to a focal idea which calls for a special reaction. The former appears as an unnoticed element in a consciousness acting as an accompaniment or mere spectator, while the latter immediately gives rise to a consciousness acting as a "guide"¹ for each detail.

Attention to details invariably made the writing more difficult after the "set" was established. The general principle seems therefore to hold, so far as voluntary movement is concerned, that consciousness exists only to get rid of itself, and that when once rid of itself the adjusted movement runs

¹Cf. Morgan: Intro. to Com. Psy., p. 189.

off best when as nearly unconscious as the conditions will permit.

This position is further supported by the fact that in no case did the "set" appear until the writing of the list of ten four-letter words had become a series of forty consecutive associated finger movements; that is, until the words, and to a degree the letters, as such had dropped out or, perhaps better, become merged with the movements and the sensations resulting from the movements, so as to be concretely indistinguishable. For example *E* reported at the close of experimentation that "general bodily sensations and tactual-kinæsthetic sensations, together with visual sensations when the eyes are open, make up the content" of the "set." This and other introspections illustrate that the gradual drift of attention is from the extra-bodily factors, with which attention is concerned at the beginning of the experimentation, toward the intra-bodily sensations as the activity becomes automatic until finally attention to details as such is lost. It is then that the tactual and kinæsthetic elements and the inner speech are observed as the only remaining sensory or ideational elements connected with the activity; and these no longer demand a shift of attention from one to the other but all enter into an organized complex with attention given to it as a whole, and without distinction of inner or outer.

This description holds only for the "practice" writing and not for writing from dictation. At the beginning of experimentation one or two of the subjects thought writing from dictation easier than from the copy, but at the close all agreed that the dictation was much less automatic than the "practice" writing. The reason for this is found in the fact that after the "set" had developed to a certain degree the dictation (a word at a time) periodically interrupted a process which had become continuous and automatic.

The dictation writing at this stage illustrates two of Ach's observations, namely, the presence of intentionalen Bewegungsempfindungen and the absence of imagery between the reception of the word and the following movement. (*op. cit.* p. 146.) In dictation the subject develops, so to speak, a special or temporary "set" for each word by running through the word in inner speech with reference to the letters on the cardboard or the corresponding bulbs or fingers, depending upon the degree of automaticity already attained. In early practice this process was repeated essentially *after* hearing the word and *before* beginning to write, but after the appearance of the "set" there was no discoverable imagery intervening between the apperception of the word and the beginning of writing. What is found is simply a psychophysical "set" for the par-

ticular word anticipated, and with the reception of the word the release of the motor consequences (temporarily inhibited) of this "set," which already contains the movement in embryo.

Inner speech continued throughout experimentation, but in the fully developed "set" it cannot be regarded as the motor cue. That it cannot by itself be regarded as the cue is shown by the fact that not infrequently the movements outstrip the inner speech for the corresponding letters. *That neither inner speech nor any other single factor can be regarded as in itself the cue is further demonstrated by frequent instances in which the movements were ahead of any sort of consciousness of the process.* In fact, with both *E* and *R* in the last days of practice the motor impulse was at times clearly ahead of all detailed conscious processes. When consciousness of the movement came into the process at such times it always came in just a little behind what the fingers were doing. But this is not to be interpreted to mean that consciousness had nothing to do with the movements and that the latter had become purely physiological. Far from it. There was a distinct limit to both the separation in time and the nature of the possible withdrawal of consciousness from the movement. If in rapid writing the movements succeeded each other so rapidly that the consequent sensations merged into a sensation-mass and inner speech became indistinguishable or some of the letters were slurred or omitted, the "set" was broken and control was momentarily lost. Again all the subjects found it possible at the close of experimentation to think of other things while writing, without interference. But here, too, there was a limit. It appears that, as one subject observed, considerable quasi-irrelevant imagery may enter consciousness without serious disturbance, provided it is all related to the writing. This irrelevant, but non-disturbing, imagery is only momentary "like a flash" or "like a dream." Otherwise the writing "set" is disturbed, if not actually broken up.

This brings us to another point in the discussion, namely the difference between the "set" with eyes open and with eyes closed. There are two differences, a difference in the rate of the writing and a difference in distractability. Some of the subjects were inclined to think, on purely introspective grounds, that they wrote as rapidly with eyes closed as with eyes open. The facts (as revealed by the special records of the "practice" writing taken for this purpose without the subjects' knowledge) are, however, that *in every case* the eye-closed practice was slower than the eye-open; and this in spite of the fact that only two of the subjects visually regarded the card-board containing the letters. This is shown in the following table of averages

for writing the list of ten words or forty letters. *R*'s averages are based upon 50 records; *E*, *C* and *D*'s upon 30 records each. The numbers express fifths of a second.

Eyes open	Eyes closed
<i>E</i> . 28.2	29.7
<i>R</i> . 35.6	39.7
<i>C</i> . 47.8	54.7
<i>D</i> . 52.5	70.2

D and *C* show the greatest differences. *D* practiced with eyes upon the cardboard in open-eye practice while *C* did not. Whatever the reason for these differences, it is clear that open eyes facilitated the writing even when the cardboard and bulbs were not regarded. It is possible that these time differences are due to differences in liability to distraction. This latter difference shows itself in the difficulty with which eye-closed practice records were obtained. Not only were mistakes more liable, but there were actual "break downs" when the attempt was made to obtain the eye-closed records, due to more pronounced consciousness of the rotating drum, etc. Such broken records were of course not used in obtaining the above averages. It is possible that the eye has a steadying effect upon voluntary movement, because of the original close connection this sense organ sustains with all our voluntary activities involving objects or extra-bodily space. It is possible, too, that vision aids by holding the external circumstances of the experiment in consciousness, at least in a peripheral way, and thus by association helps to maintain the "set." At any rate the "set" of consciousness here described was more readily maintained in eye-open than in eye-closed practice.

To summarize, The automatization of a highly voluntary movement like the one here studied, involves the three following stages:

1st. A period of perceptual attention to details in which vision plays the leading rôle, the muscular sensations being only marginal, unless abnormal conditions, subjective or objective, force them into the focus of consciousness.

2nd. A transitional period, or period of adaptation, which is characterized by three things: (a) A process of elimination of perceptual data and, in instances if not in all cases, the substitution of memory imagery, for a longer or shorter period, for the eliminated perceptual data. (b) A gradual lessening of the intensity of attention and of general psychophysical tension. (c) The development of an organized "set" of consciousness in which the details no longer require attention in themselves but only as parts of a complex whole, the elements

of which are sensuous, rather than perceptual, but have yet the *motor* significance of perceptual or sense data attended to.

3rd. A period in which the volitional part of the process consists only in the maintenance of the "set," the details which enter into it taking care of themselves, provided the "set" is not disturbed beyond a certain degree of habitual fluctuation.

The bearing of these experiments upon the general nature of voluntary movement may be summarized as follows:

(1) Where a movement involving external objects is new and highly volitional, attention is normally objective, dealing with *percepts* of the *immediate*, rather than with *images* of *past* movements.

(2) Those sensations are functionally most important which reveal the present situation most faithfully in accordance with the tendencies of the individual and the exigencies of the situation.

(3) Kinæsthetic sensations become prominent, but do not *necessarily* take on guidance value, when the situation is complex and stimulating.

(4) It is only when a certain degree of skill and proficiency has been reached that imagery (in contradistinction to percepts) of any sort becomes important. This imagery was uniformly derived from the preceding perceptual experiences in making the movements (voluntarily) and *not from reflex and involuntary activities*. In other words this imagery has a *conscious* source. The experiment affords no evidence for such a thing as imagery derived from purely reflex and unconscious experiences.¹

(5) It does, however, afford evidence that much of the control imagery as such fades away with practice and that in the last stages of a voluntary movement a complex idea of the general situation, with little or no *particular* imagery of a clear character, is sufficient to carry on a series of practiced movements. In this stage the sensations arising from the movements themselves do not necessarily give rise to perceptions, but perform their proper functions without the aid of supplementary ideational processes.

(6) There is some evidence that this complex idea or "set" of consciousness may in the case of such movements as those of type-writing be purely verbal so far as its image aspect is concerned, although the eye-closed writing gives evidence that concrete sensory processes facilitated the writing even at the end.

¹It must not be forgotten, however, that the movement of pressing a bulb (as would in fact be true of almost all movements of adults) was not a wholly new muscular performance but rather a special adaptation of a movement already at command in a less specific form.

II. *Writing Experiments, Involving Different Degrees of Practice and Volitional Control*

The tests used were the following: (a) Copying a list of familiar English words. (b) Copying two-syllable nonsense words. (c) Copying German words. (d) Copying the English words while tapping with the left foot. (e) Copying the nonsense syllables while repeating the Greek or German alphabet. (f) Writing the subject's name and address upside down with the left hand. (g) Writing the name and address with the left hand while shielded from direct vision and controlled by means of the reflection of it in a mirror placed before the paper.¹

With the exception of the upside-down writing and the mirror writing, which were not timed, one minute was allowed for each exercise. In all eight subjects (graduate students in psychology) served for these experiments, each subject taking the seven tests at least twice and in some instances three times. The general uniformity of the results warranted stopping this set of experiments with this limited number of tests.

In the discussion of the writing experiments it may be well to call attention at the outset to the physiological complexity of the movement. Physiologically writing is a highly complex activity involving the co-ordination of movements at six joints from the shoulder to the first joint of the fingers. With these movements, so intimately related to one another, must also be correlated the movements of the eyes and to an extent those of the head and body. Our problem is how these co-ordinations are controlled. It is to be observed at once that we are not immediately aware of any of the co-ordinations mentioned. It is only by reflection upon the act of writing that we become aware of them in any definite sense. Another palpable observation is the fact that writing involves fineness of movement and co-ordinations and therefore fineness of discrimination somewhere on the side of control, for it may with safety be assumed that sensory discrimination, conscious or unconscious, is directly related to accuracy and delicacy of movement and it is certainly conscious in some measure until the movement becomes habitual or automatic.

If then, with this delicacy of movement and fineness of sensory discrimination in mind, we ask what on a *priori* grounds is the sense organ that primarily controls such move-

¹It is only fair to state that these experiments were complete before the publication of the somewhat similar ones of Dr. Downey mentioned in the preceding section.

ments, especially in its early stages, the eye at once suggests itself. In the experiments here described the tendency to use the motor cue as the focal element is nowhere in evidence except in the upside down and mirror writing. Muscular sensations are occasionally mentioned in the other tests but never reported as focal. Moreover, it is very likely that the introspective attitude was influential in rendering them focal in the tests in which they so appear. A comparison of the tests in which kinæsthetic sensations appear as focal brings out an interesting fact. When, *e. g.*, in the writing with mirror guidance, interpretation of the visual process is made necessary by the conditions of the experiment while the movements themselves are in no wise changed from normal writing, the subject falls back upon the use of motor sensations as the focal elements almost in spite of his effort to the contrary. That is, the tendency to follow *directly* the visual cue given by the mirror and thus make the wrong movements is so strong that to write correctly requires the neglect of vision until a new co-ordination is started when the mirror cue can be used directly. This, we think, shows clearly what a predominant function vision normally assumes in our writing reactions, especially when beginning.

In the upside down writing *the* difficulty was to visualize the *letters* upside down, *not* the movement nor to have an anticipatory image of how the changed movement was going to feel. This primary difficulty of visualizing the letter in the reversed position was experienced by every subject, but was met and overcome in various ways by different subjects. Some succeeded in visualizing the letter upside down, two visualized themselves as sitting "on the other side of the desk" while one thought "of how the hand should go" and of the muscular sensations when the movement was once started. Attention is called to the fact that in the mirror writing (*i. e.*, writing with work reflected in the mirror) what is being put on the paper is constantly flashed back to the eye in a reversed position, and that the *sight* of this reversed position tends immediately to reverse the movement, while in the upside-down writing the subject sees what is written *as* it is written. In the latter the vision controlling the movement is direct and correct; in the former it is indirect and reversed. In the upside-down writing the muscular sensations were theoretically radically changed from normal writing while the subject's vision of what he was doing was not interfered with, but in the reflected writing the muscular sensations theoretically were the same as under normal conditions and actually should be the same so far as the movement itself is concerned, while the subject's vision of what he was doing

was radically changed, making a condition in essentials the reverse of the upside-down writing. It is therefore clear that in writing movements the muscular element can be changed (not eliminated) with less interference with the activity than is produced when the visual element is changed.

This suggests that on the sensory side writing may be regarded as a visual-kinæsthetic activity with vision representing the extra-bodily aspect of the total process and the muscular sense the intra-bodily aspect. As a matter of fact it would seem that every new voluntary activity whose object is an extra-bodily end requires the sensory or perceptual representation of those two aspects of the process and these two only, the one representing the *status quo* of the thing being manipulated, the other that of the bodily member immediately concerned with the manipulation. Our writing experiments show that in the more highly volitional tests attention is uniformly upon the extra-bodily aspect unless conditions are such that reaction to the visual elements can take place only by way of complex interpretation. The study of the cases of anæsthesia (to be described later) will show that the muscular sense normally serves the function of giving the *status quo* of the bodily member, but that in case of tactual-kinæsthetic paralysis vision is called upon to perform this function. These facts agree with the facts obtained in the present study. Moreover the facts there obtained agree also with those obtained in the writing experiments in this, that in the automatization of voluntary movement the extra-bodily elements drop out of consciousness before those representing the position of the member in movement. This is abundantly illustrated by the fact that every experiment with the upside down and mirror writing shows attention to the result, and not only that, but *visual* attention wherever it is practically possible. On the other hand in those experiments in which the movements and the manner of control were familiar but the copy unfamiliar and meaningless, as in case of the nonsense syllables, there was a minimum of attention to the result. The same thing is true of copying nonsense syllables and repeating the Greek and German alphabet. In this case attention fluctuated from the copy to the alphabet but not to the result put on paper. In both these cases the handwriting suffered somewhat, as is shown by comparison with the other experiments in which there was visual attention to the result. It suffered not only in the large co-ordinations such as following the line, spacing, etc., which involve a spatial reference for which the muscular sense is inadequate, but also, in instances in a marked degree, in the details of the movements which apparently have "gotten into our muscles" and which

in normal writing under visual guidance receive practically no attention. Ordinarily we write with the eye focussed upon the paper at or near the point of the pen while attention may be, and usually is, far away from the movement as such. In fact, consciousness of the finer aspects of the movements involved seems to be next to nothing, yet the slight peripheral visual consciousness (it cannot be called attention) is evidently of great value in furthering the co-ordinations.

In such controlled reactions as copying words under conditions involving varying degrees of difficulty there are three things we *may or must attend to or be conscious of*, depending upon the conditions and the difficulties involved. These are the copy, the result, and the position and movements of the bodily parts active in the production of the movements. Attention to, or consciousness of, the first two is always visual. It is moreover highly probable that in the *early* stages of writing the visual *attention* fluctuates between these two and the third. The position is, however, never normally *attended to* without resulting in distraction. Of these three factors the *details* of the result, as already shown, drop out of consciousness comparatively early. It is precisely these details which in the early stages of learning to write give the most difficulty and which are attended to most closely. It is here tentatively suggested that this principle holds for a large range of our acquired activities. This much seems clear, that consciousness of position either by means of the resident sensations or by means of the visual and remote sensations is necessary long after the details of the result have dropped out. It is highly probable that in coördinations that are far removed from our inherited reactions, like writing, consciousness of position can never drop out with impunity. The long continued practice that is necessary to reestablish walking (which at most is only partially a learned reaction) without the aid of the eye, in cases of sufficient tactual and kinæsthetic anæsthesia to destroy the sense of position, is strong presumptive evidence in favor of such a view.

Two other considerations of interest with reference to the automatization of writing movements are the behavior of inner speech and the appearance of more or less irrelevant imagery as the process becomes easier. The inner speech, more generally mentioned than any other one thing, is universally present in the copying of nonsense syllables and German words. The introspections contain only two observations of it in the upside down and mirror writing and these by the same subject. But this fact, we believe, does not justify the conclusion that these instances show that it was not generally present. It seems probable that if present, its infrequency, due to the slowness of the

writing and the objective nature of attention, might easily account for its being overlooked in the introspections. This much, however, seems clear, that inner speech rises into distinct consciousness whenever the *meaning* of what is being written is not at once present. Whenever the ideational processes resulting in or constituting meaning are blocked, inner speech at once comes into clear consciousness. And probably also when anything hinders attention to the writing. But, although the introspections are inconclusive upon this point, there are indications that, in spite of its greater prominence, inner speech is little more than an accompaniment when the copy and conditions are unfamiliar and difficult and that it is more distinctly a cue when the copy is familiar and the conditions easy. Evidence for this is found in the constancy with which the eye is kept upon the copy when the former conditions prevail and its desertion of the copy in the latter conditions. That is, when the eye has to be kept upon the copy the writing becomes more nearly an act of drawing; in fact that is what it amounts to in the upside-down writing where the primary difficulty is one of visualization.¹ If this view is correct, it follows that inner speech unaccompanied by ready meaning, does not in itself serve as a cue. The inner speech, in this case is probably primarily of service in the development of meaning and not in the direct control of the movement. More exactly it is not so much a control factor as an initiatory element; it is the element with the appearance of which meaning is primarily developed.²

The facts brought out in this set of experiments tend to show in connection with the preceding experiments, that in a general way the content of consciousness *changes* rather than diminishes

¹The copy was, of course, *not* presented upside-down.

²It is, however, not to be assumed that the same explanation is offered for the function of the auditory image (which is always present in inner speech and constitutes an aspect of the latter) in the case of actual speech. That the auditory image has a direct control over speech regardless of meaning in any specific sense is readily demonstrated by the child's ability to enunciate more correctly than most adult beginners the sounds of an unknown language. That the auditory image is *primary* in such control is also evidenced by the methods employed in teaching deaf mutes to speak. Cf. Farrar: *Arnold's Ed. of the Deaf*, pp. 127-128. This position is uniformly supported by reports of methods employed in teaching deaf mutes to speak at several institutions for the education of this class of defectives. This statement from one of them is illustrative. "Absolutely deaf children always remember the visual appearance of the word on nose and lips and associate this with the feeling produced in them when they give the same sound. Only the blind deaf make a constant practice of touching another's lips. Those who have some hearing left, probably have an idea of the sound of the word when they speak it. This largely takes precedence of all other impressions."

as movement becomes easier. The decrease of associations and the growing absence of irrelevant imagery as the subject goes on from the copying of simple English words up to the upside-down and reflected writing (in which they do not occur at all) shows that as attention becomes more necessary and more concentrated the range of consciousness narrows; and the converse is also true, as attention becomes less concentrated, the range of consciousness becomes wider, and more or less extraneous imagery appears. It is therefore clear that in case of easy movements attention responds more readily to other stimuli and as a consequence becomes a mixed process, including, so far as the movement is concerned, both relevant and irrelevant elements. It would therefore be easy to fail to see the woods because of the trees and report no imagery when in reality it may have been present. As a matter of fact there are here two things to be kept distinctly in mind; the first is that in highly voluntary movement, but not too difficult to permit *Zerstreutheit*, attention is intense and consciousness narrow, and therefore the imagery or sense data are strictly limited *at any given pulse of attention*. The second fact is that in well learned and facile reactions the attention to the activity is minimal and the range of consciousness correspondingly wide. Strictly speaking it is therefore not true, at least from the point of view of the objective movement which it produces, that consciousness is always a unitary process, for clearly the most important elements in the configuration of consciousness may not always participate in the production of a specific movement.

III. Anatomical, Physiological and Pathological Data

It is the purpose of this section to review, as succinctly as the complexity of the subject will permit, the leading anatomical facts and physiological experiments, together with a number of pathological cases of anæsthesia, that have a bearing upon voluntary movement.

1. The Anatomical Facts as reflected in Recent Neurological Literature.

Recent neurological studies on the relative number and distribution of sensory and motor fibres in the peripheral nerves throw a certain light upon the relation of sensory impulses to voluntary movement. Donaldson has pointed out that, "Taking the central system segment by segment, the sensory nerves are more numerous and have a greater area than the motor. In man, as we pass cephalad, the superiority of the sensory nerves becomes most marked,—"(Growth of the Brain, p. 196.) Ingbert counted the nerve fibres of the left dorsal roots of a man weighing 180 lbs. and found the total number for the left side to be 653,627 which would give a total, for both sides, of

1,307,254, assuming that the innervation is the same for both sides. Ingbert also counted the fibres in the ventral roots of the same cord and found the total number, for both sides, to be 407,400. This would make the ratio of sensory to motor greater than 3:1 and at once suggests James's figure of a "funnel" with the big end representing sense processes and the small end movements.

Of still greater interest from a psychological point of view is Ingbert's table showing the number of fibres for each spinal segment. The curve for these figures shows in a striking manner that the number of fibres and the area of the nerves representing sensation from the more mobile and voluntarily controlled parts of the organism, particularly the arms, are very much greater in proportion to mass than they are for the less controlled organs. While there are no data demonstrating it in detail, roughly and in general, at least, one may say that sensory innervation is directly proportional to mobility and voluntary control. This position is supported by the experiments of Van Biervliet on "*Le toucher et le sens musculaire*." (*L'Année Psy.*, 13, 1906, pp. 114-121.) Van Biervliet found that sensibility to touch is directly proportional to the mobility of the organ and that sensitivity increases with practice and skill in control.

Further light is thrown on these anatomical facts by Head's recent study of peripheral sensibility. ("The Consequences of Injury to the Peripheral Nerves in Man." *Brain*, 1905, pp. 116-338, and "The Afferent Nervous System from a New Point of View." *Ibid.*, pp. 99-115.) Head distinguishes three forms of peripheral sensibility:

I. "Deep sensibility, capable of answering to pressure and to the movements of parts, and even capable of producing pain under the influence of excessive pressure, or when the joint is injured. The fibres, subserving this form of sensation, run mainly with the motor nerves, and are not destroyed by division of all the sensory nerves to the skin." (p. 111.) "Deep sensation is not materially affected by the destruction of all the nerves to the skin, and it must reach the central nervous system by fibres that run in other channels than the so-called sensory nerves." (p. 215.)

II. "Protopathic sensibility, capable of responding to powerful cutaneous stimuli, and to the extremes of heat and cold. This is the great reflex system producing a rapid, widely diffused response, unaccompanied by any definite appreciation of the locality of the spot stimulated." (p. 111.)

III. "Epicritic sensibility, by which we gain the power of cutaneous localization, of the discrimination of two points, and of the finer grades of temperature, called cool and warm." (p. 111.)

In an article "On the Anatomical Constitution of Nerves of Skeletal Muscles." (*Journal of Physiology*, Vol. 17, pp. 211-258.) Sherrington reports a study of sensory and motor fibres supplying different muscles. He says: "Taking the whole series together, the average proportion of the afferent fibres to the total myelinate fibres in the nerves of the muscles examined proves to be a little less than a half (49%). The proportion of afferent fibres to the total myelinate fibres ranges from a little more than a third in some muscular nerves to a full half in others." (p. 229.) "In the nerves to some muscles the afferent fibres are as numerous as the efferent. It is probable that the very largest cells in the spinal ganglia belong to some of the nerve-fibres of the muscle spindles. Probably in every spinal ganglion a number of the nerve-cells belong to the sense organs of muscles." (*The Spinal Animal. Medico-Chirurgical Translations*, Vol. LXXXII, p. 455.)

Dunn caused all the motor nerves to the legs of a frog to degenerate and "found that there remained an ample supply of medullated fibres to the muscles. These represent from 15 to 30 per cent. of the fibres going to the muscles."

Assuming that the relative motor and sensory nerve supply to the voluntary muscles of the guinea pig and white rat is approximately the same as it is for the animals studied by Sherrington and Dunn, we have in their results an excellent neurological basis for the psychological results obtained by Small and Watson with the rat and Allen with the guinea pig. (Watson, *Mon. Sup., Psy. Rev.*, May, 1907. Small, *Am. J. Psy.*, Vol. 12, pp. 206-239. Allen, *Journal Comp. Neur. and Psy.*, Vol. 14, No. 4.)

Both Small and Watson, especially the latter, found that kinæsthetic processes play a highly important, if not a preponderating rôle in the controlled movements of the rat when learning to run the maze. Allen found that in case of the guinea pig "vision is an important element" but that the "labyrinth is not learned solely, or even largely, in terms of tactual sensations." Her conclusion is that "kinæsthetic sensations are of great importance in the recollection of a path." (*Op. cit.*, pp. 330, 336 and 337.) Yerkes working with the frog came to practically the same conclusions, finding that "beyond question vision and the direction of turning were all important factors in the establishment of the habit." (*Harvard Psy. Stud.*, I, p. 579.)

In connection with what Allen finds to be true with the guinea pig, it is interesting to recall that frequently in human cases of locomotor ataxia the cutaneous sensibility escapes injury while the muscular sense always suffers more or less and

with a corresponding effect upon co-ordination. Head's cases on the other hand clearly illustrate that the loss of *tactile* sensations alone in man has little or no effect upon voluntary movement.

2. Physiological Experiments.

In order to determine the effect of the loss of "resident sensation upon movement, Mott and Sherrington working on the monkey severed the dorsal roots on one side of the cord supplying the arm and leg with sensory innervation, viz., the 4th cervical to the 4th thoracic inclusive, and the 2d to the 10th post-thoracic inclusive. The results are described in their own language as follows: "From the time of performance of the section onward as long as the animal may be kept [they kept them about four months] the movements of the hand and foot are practically abolished. On the other hand, the movements of the elbow and knee, and especially the movements at the shoulder and hip, are much less impaired. If the feeding time be deferred, and an animal, in which the apæsthete limb is an arm, be tested by offering it fruit after the sound arm has been secured behind the back, there is no attempt to use the apæsthete limb for reaching the food, but the neck is thrust forward in order for the mouth to seize it." One monkey tried to take food with the apæsthete foot but failed,— "the digits were not moved." "The defect in motility increases from the attached base to the free apex of the limb; so that, for instance, while comparatively slight at the hip, it is successively greater at the knee and ankle, and greatest (amounting as regards volition, to absolute loss) in the digits." The authors conclude that volition "has been absolutely abolished by the local loss of all forms of sensibility." (*Proc. Roy. Soc.*, Vol. 57, pp. 481 ff.)

Bastian, on the other hand, while accepting these facts criticises Mott and Sherrington's interpretations. *Brain*, Vol. 18, pp. 609 and 615. Bastian argues that limb movements may be paralyzed or rendered defective in two ways: (a) by organic lesions (b) by functional defects. These functional defects are of two sorts: in the one case the "seat" is cerebral, in the other, spinal. He says that "in cases of complete hemianæsthesia due to lesions or functional defects in the posterior part of the internal capsule, there is not only no paralysis, but little or no impairment in the ability to perform, under visual guidance, even the most delicate movements with the apæsthete limbs." As is well known, Bastian holds that there are no motor centres strictly speaking, that what we call motor centres are essentially kinæsthetic centres and that impulses have to pass through these centres before a movement can be made, but that these impulses may originate in the auditory centre

as in speech or in the visual centre as in hand movements, and therefore there may be controlled movements without kinæsthetic sensations from the limb to be moved.

Sherrington, however, writing after further experimentation bearing upon the relation of sensation to movement seems to modify somewhat his original rather rigid and sweeping position. He writes: "It is found that the destruction of sensitivity in particular regions brings about objectively observable disturbances of movement." . . . "the effects of apæsthesia upon the musculature of the part are three—(1) paralysis, (2) ataxia, (3) atonia." (*Proc. Roy. Soc.*, Vol. 60.)

Munk repeated the experiments of Mott and Sherrington and at first obtained the same results, but later found that monkeys recovered to a degree their ability to use the hands that had been rendered anæsthetic. These movements, however, were always inaccurate and exaggerated. Moreover only single voluntary movements could be made at all. Munk's animals at first dropped food placed in the anæsthetic hand but later learned to grasp it and carry it to the mouth. After getting it to the mouth the monkey at first removed the fingers from the food with his teeth but after some practice learned to let go "actively" when the hand reached the mouth. Then instead of placing the food in the monkey's hand, Munk simply presented it to him. With the sound hand tied behind him the monkey after experience learned to take the food from Munk's hand and carry it to his mouth with the anæsthetic hand.

What Munk specifically denies in Mott and Sherrington's deductions from these facts is that the entire sensory mechanism is and must be operative in voluntary movements, that the entire sensory path from the periphery to the cortex is necessarily active in volitional control. Munk points out that the only movements that are entirely and permanently destroyed are the reactions that normally take place to an immediate stimulation of the member concerned. All other movements which the extremity normally makes are more or less defective because the "*Einstellung der centralen Organe von welchen die Bewegungen der Extremität herbeigeführt worden, verändert ist. Die Störung ist immer desto grösser, je mehr Muskeln oder Glieder der Extremität an der Bewegung beteiligt sind.*" The important differences between Mott and Sherrington and Munk appear to lie in the fact that the former seem to regard kinæsthetic elements as the essential sensory elements in the voluntary arc, while the latter holds that any sense elements, visual, auditory, etc., will serve the same purpose quite or nearly as well in the absence of kinæsthetic elements, especially after practice.

It will be recalled that Sherrington found that in the spinal frog "the initial posture of the limb distinctly affects the character of the reflex movement—even in the absence of cutaneous organs." This fact, together with Munk's results and criticism of Mott and Sherrington, takes us again to Judd's position that in the animal relatively low in the phyletic scale the kinæsthetic areas play a more important function than they do in the higher animals for the reason that in the latter the special senses have become more highly differentiated and have accordingly, to a degree at least, usurped the original primacy of kinæsthetic centres and therefore play a more important part in volitional activity. However hypothetical Judd's position may appear, it certainly possesses the advantage of facilitating interpretation, an advantage which warrants its application. If we assume that the original undifferentiated form of sensation is of the tactual-kinæsthetic type and that from this original type all other forms of sensation have been gradually differentiated, it is clear that in animals low down in the scale of evolution the original type of sensation must necessarily perform the important function of *the* sensory cue for voluntary movement. On the other hand, in animals high in the same scale the other sense processes are so highly specialized and developed that, when measured in terms of relative discrimination, they far surpass the original type from which they took rise, and may now play the principal rôle in voluntary action. Moreover if in making a new and conscious adaptation—which involves discrimination and choice and therefore voluntary movement—the important thing is the correct grasp of the situation rather than the consciousness of the movement as such, then it also follows that the sense organs of later development naturally play a primary rôle for the reason that their discrimination is finer, and for the additional reason that they act at a distance.¹

(3) Pathological Cases. Disease has done, not infrequently, what is impossible to experimentation upon human beings; it has destroyed the sense processes, and given us the general relation of these processes to voluntary movement in very much the same way as the physiological experiments of Mott and Sherrington and Munk reveal them in the case of the monkey. It is therefore in place to review here a number of typical cases of anæsthesia and to attempt an interpretation of their bearing upon voluntary movement. One naturally begins with Strümpell's celebrated case. Of the extent of this boy's anæsthesia James says that he "was totally anæsthetic without and within

¹ Cf. Sherrington: *The Spinal Animal* (*Medico-Chir. Trans.*, Vol. LXXXII, p. 475.)

save for the sight of one eye and the hearing of one ear." (Principles of Psy., Vol. I, p. 376.) It appears, however, from Strümpell's own account of the case quoted by James, that in "violent forced hyperextension of the joints, especially of the knees, there arose a dull vague feeling of strain, but this was seldom precisely localized." (Prin. of Psy., Vol. II, p. 489.) These poorly localized, kinæsthetic sensations seem, however, to have been of some service, for Strümpell tells us that with his eyes closed, the boy could readily raise his arm at command. Another anæsthetic case of Strümpell's is cited by James in which "The arm could *not be moved at all* [italics James's] unless the eyes were opened, however energetic the volition." (Prin. of Psy., Vol. II, p. 491.)

In "Ein Fall von aphasischen Symptomen, Hemianopsie, Amnesthetischer Farbenblindheit und Seelenlahmung, (*Archiv für Psychiatrie und Nervenkrankheiten*, Vol. 25, 1893.) Bleuler describes a case of total anæsthesia on the right side. Of its effect upon movement he says: "If the patient does not see his right arm, he not only does not know where it is, *but cannot innervate it at all.*" In summing up his observations upon these phenomena Bleuler says, "It is known that patients who are not at all aware of the effects of their muscular innervation are no longer in a position to make spontaneously purposive movements." He cites the case of Niemeyer and Späth whose patient, an "Anæsthetische Tabetiker," fell down while standing or sitting if he closed his eyes. He cites also the case of Ziemssen and Heyne, whose patient was entirely anæsthetic with the exception of his face and hearing, and who could not speak when his ears were stopped.

A. Pick describes a case (Ueber die Sogenannte 'Conscience Musculaire,' *Zeit. für Psy. und Phys. der Sinnes*, Vol. 4, pp. 175 ff.) in which the patient (a young woman) presented 'eine nahezu die ganze Körperoberfläche einnehmende Anästhesie und Analgesie.' On the right side there was complete loss of both superficial and deep sensibility. In all experiments involving hand movements this patient sharply fixated her right hand, which was completely anæsthetic. This fact, Pick points out, does not agree with Binet's (apparently *a priori*) contention that, other things being equal, attention would be fixed by preference upon the sensible half of the body, because this is the only part which gives the muscular sensations during movement. Pick's patient was unable to hold the left arm horizontally extended and at the same time make the movements of playing the piano with the right. She could, however, hold the right hand extended and make such movements at the same time with the left with-

out serious disturbances. If with eyes closed, the patient is asked a question she merely moves her lips in reply but upon opening her eyes declares she answered the question aloud. If her ears are closed and she is handed a written question she again merely moves her lips in reply, declaring when her ears are opened that she replied aloud. If she is requested to open and close her hands she accomplishes the movements by fixating the right hand. But if the right hand is placed outside the narrow field of vision the movements cease. Two interesting facts appear in this case: (a) when the sense processes are reduced to a certain minimum the movements called for, if executed at all, are reduced to a certain minimum of intensity. (b) In the two hand movements the left hand is controlled by means of the "resident" sensations while the right hand is controlled by means of vision or "remote" sense processes. In other words, either resident or remote sensation is essential for the control of each arm, the coördination (apparently) taking place when the one is controlled by "resident" and the other by "remote" processes.

Pick also cites the cases of Duchenne who observed three patients with total loss of sensibility on one side. Duchenne found that they were unable to move the anæsthetic members without the aid of vision. Both Pick and Duchenne explain their cases wholly in terms of attention. This explanation may be criticised on the following grounds: (1) Attention is not a psychic entity, as these authors appear to imply, but only a state of psychic processes. (2) As a state of psychic processes attention is, to quote Pillsbury, merely "an increase in the clearness of one idea or group of ideas at the expense of others." (Attention, p. 11.) (3) To attend there must be something to attend to, which is what is wanting to the anæsthetic. That attention is an important factor in all forms of voluntary movement would probably be conceded by every competent observer, but that attention cannot create sensations and ideas but only render them conscious, or better, more clearly conscious, must be *conceded with equal readiness*. And what the facts show in these cases is *primarily* a loss of sensations and ideas normally functional in voluntary movement.

Bleuler also rejects the explanation in terms of attention on the ground that his case showed no marked disturbances of attention *when getting sensations*. In place of this explanation Bleuler offers the following. He points out that we never take notice of the particular muscles concerned in making movements. "Unser bewusster Wille innervirt also nicht bestimmte Muskeln, sondern er führt mit den Gliedern bestimmte Locomotionen oder bestimmte Thätigkeiten aus."

"When we reach for an object we move the hand to the right or left as may be necessary, but in turning the hand to the right the opposite muscles are used from those used in turning to the left. But in all such movements the conscious process, the desired activity remains the same." Bleuler holds, too, "gewisse Kenntniss der Ausgangsstellung ist also zur Ausführung der meisten bewussten Bewegungen unentbehrlich."

Berkley reports "A case of General Cutaneous and Sensory Anæsthesia" (*Brain*, Vol. 23, pp. 111-138), in which, besides total blindness and partial deafness, there was almost total loss of the two forms of sensibility called by Head Protopathic and Epicritic but with only partial loss of "deep" or muscular sensibility, *i. e.*, there was total loss of "thermic, pain, olfactory, gustatory, equilibrium, pressure and weight sensations," and almost total loss of the visual but only partial loss of the muscular sense. "With auditory perceptions a progressive dulling could be noted." In fact "none of the special senses or cutaneous sensations remained wholly uninvolved." Considering the degree and extent of anæsthesia, this case presents two striking considerations: (1) "The musculature, while responding to the will, did so in such a feeble manner that the patient was incapacitated from helping herself to any extent. Thus the dynamometer, when taken in the hand and squeezed, was so feebly compressed that the indicator showed no movement of the dial, though the woman exerted every effort in the trial." This in spite of the fact that both nerves and muscles responded promptly to the galvanic and faradic currents, and in spite of the fact that the autopsy revealed no emaciation. (2) The second fact to be noticed in this case is that "throughout the long course of the illness there was never the slightest departure from normal mentality on the part of the patient." It is therefore clear, that so far as this case is concerned a general loss of sensibility affects voluntary movement more seriously than it does the ideational processes.

Spiller reports a case of anæsthesia in which a loss of muscular sensation resulted in inability to control the member unless it was kept in the field of vision. The "sensations of touch, pain and temperature are diminished in the left, but not to the same degree as are the sense of position and stereognostic perception." ("Separate Sensory Centres in the Parietal Lobe for the Limbs," *Journ. of Nerv. and Mental Diseases*, Vol. 33, pp. 117-121.) The defects of movement accompanying these sensory disturbances are described by Spiller as follows: "The movements of the left upper limb are awkward, although the limb can be moved freely at all

parts, and is not weak. The patient is unable to button his coat with his left hand alone when his eyes are closed, because of incoördination of the fingers, but can button it promptly with his right hand." (pp. 119-120.)

F. Pick describes a case which, besides other complications of partial anæsthesia, presented total ("constant totale") anæsthesia of the left side. While unable to imitate with the right leg passive movements of the left (anæsthetic), this patient could under visual guidance readily imitate with the left leg passive movements of the right. With the aid of vision he could, though hesitatingly, raise the left arm, but with eyes closed he was entirely unable to do so. However, if one passively bent the right arm (the eyes being closed) and repeated the command to bend the left or anæsthetic arm, the command was "promptly" obeyed and the movement made "in der gleichen Weise." (Ueber Transcorticale Störungen des Bewegungsapparates, *Deutsches Archiv für Klin. Med.*, Vol. 76, pp. 144 ff.)

Hoppe describes a case of almost complete anæsthetic paralysis of the right arm, together with "well marked amnesic aphasia," besides other more or less complete disturbances of sensibility. (Soul Paralysis, *Jour. of Mental and Nerv. Diseases*, Vol. 32, pp. 145-159.) With reference to the sensibility of the arm the author says "the muscle sense and sense of position of limb" are "almost absent." Muscular power, however, is "almost normal." The effect of anæsthesia upon movement is described as follows: "The arm is completely paralyzed, has not moved at all during the four weeks since onset of trouble. The arm is flaccid, relaxed, reflexes are absent, upon being requested to move the finger or hand, was not able to make the slightest movement, although she made an attempt to do so, showing that she understood the request. Very often, later on, a request to move the right hand was answered by the movement of the left hand. If the patient, however, was told to watch the doctor's hand and then to repeat the movement, the movement could be executed at once. Then the apparently completely paralyzed hand was made to extend and flex the fingers and thumb, to spread them wide apart, and to flex and extend the hand on the second day. Inside of a week all the normal movements of the arm and fingers—flexion, extension, moving of arm and placing hand on head and elevating shoulder could be done, usually only after she first saw the movements executed by the physician, and only then when she was told to watch the movement closely." (pp. 147-8. *Italics mine.*) With reference to the arm, Hoppe's résumé of the sensory and motor disturbances in this case is as follows: ". . . right sided diminution of sense of

touch, pain, muscle sense, and sense of posture. Condition of motor function: an absence of real paralysis in right arm and leg, but the arm is not used spontaneously for voluntary acts: lies as if paralyzed along side of patient; arm is used for reflex acts, like scratching, and for unconscious acts, like bracing herself when arising from a sitting or recumbent position to a standing one. When shown how to make a movement, and especially when the movement is made passively in the arm several times, the patient is able to repeat it perfectly. These are then apparently normal and not clumsily made *nor is it necessary for patient to control these movements with her eyes.*" (pp. 148-149. Italics mine.)

Here we have a case which on the mental side illustrates three sorts or grades of movement:

(a) The highly volitional. Because of amnesia and lack of practice (it will be recalled that for four weeks the arm was not moved) this patient had apparently entirely forgotten certain arm and hand movements. These movements were made again by means of visual sensation from, and attention to, similar movements first made by the physician and also by means of visual sensation from, and attention to, the arm itself while the movements were being made.¹

(b) The practiced voluntary. After practice, it was observed, the patient could make these arm movements without immediate visual control. (It is, however, always necessary to remember that the arm was not completely anæsthetic.)

(c) The reflex instinctive (?) These movements ("scratching," "bracing," etc.) did not require a period of learning and practice before they could be made without imitation and visual control, but appear to have been carried over into the anæsthetic condition from the normal condition. For these the remaining kinæsthetic sensibility seems to have supplied a sufficient sensory cue for their ready performance.

F. Müller describes a case of right-sided anæsthesia so nearly total that the patient could not distinguish passive movements

¹Another instructive case has been kindly supplied me by Dr. Fairbanks of N., Mass. The patient, a young woman who suffered from spinal meningitis when ten years old, has lost sensibility in the fingers of both hands and cannot do anything with her fingers unless she can see what it is; for example, she can button or hook a garment that opens in front but not one that opens in the back. She can put her hat pins in if she is in front of a mirror. After anæsthesia appeared (which was not until about fifteen years of age) the attending physician recommended horseback riding, but this had to be given up because the patient could not hold the reins unless they were constantly kept in the field of vision. This case is particularly significant, because there are no other complicating disturbances, the patient being in all other respects highly efficient.

except when very intense. "Die aktive Bewegung der Hand und der Finger war nicht vollkommen aufgehoben aber ungeschickt, alle feineren Bewegungen waren unmöglich." (p. 403, Ueber Störungen der Sensibilität bei Erkrankungen des Gehirns, Klinischer Vorträge, Serie XIV, Heft 4-5, pp. 337-451.) In this case, too, visual sensations from the fingers were necessary for use of the finer muscles. But the significant thing in this case is that while at the end of two years the patient (an intelligent person) could not tell the position of his arm with eyes closed, he had nevertheless learned to use his hand with some facility.

The case cited in the note above (anæsthetic for 15 years) tends to show that when the anæsthesia is total no amount of practice will bring back control of the finer muscles without visual sensations from the member. The cases of Müller and Hoppe also tend to show that when local sensibility is still present, though so reduced that vision must be employed for a long time, a slow process of training under visual control may again re-establish control without visual sensations. This is well shown by Goldscheider in his "Anleitung zur Uebungs-Behandlung der Ataxie" in which he gives in great detail many devices for the re-establishment of walking and of hand and arm control, ultimately without visual guidance. It appears, however, from Goldscheider's account that there is strong presumptive evidence that none of the cases succeeding without visual assistance are totally anæsthetic. The fact seems to be that in cases successfully treated there is a modicum of sensation left which attention gradually gets hold of, and thus gradually regains control.

(4) Summary and Conclusion. The study of the proportion of sensory and motor fibres in the higher vertebrates including man shows not only a great preponderance of sensory fibres in general, but also a rich supply of sensory fibres to the voluntary muscles. The curves giving the distribution of the number of sensory and motor fibres, for the ventral and dorsal spinal roots show clearly that in a general way at least there is a marked increase in the number of sensory fibres supplied to regions whose muscles are most used in controlled activity.

Correlated with these facts are the studies of animal activity tending to show, in some instances by a process of elimination, that the kinæsthetic processes play an important part, if not a predominant rôle in the controlled activities of lower animals. Sherrington appears to refine upon this position somewhat by holding that those sensations giving the location of the member are *the* essential ones. He says: "It is found that kinæsthetic sensations of the movement to be acquired or controlled, though helpful, are less important than the resi-

dent sensations from the part in its 'resting' state. These latter, with the power to focus attention upon them, appear to be a most necessary condition for the acquirement of control. And in the monkey, voluntary control of a limb is largely lost when the limb has been rendered apæsthetic." (The Integrative Action of the Nervous System. p. 390.) Other physiological experiments support the same thesis. Munk's experiments tend to show, however, that in case of the monkey after loss of kinæsthetic sensibility there may be a process of relearning under guidance of vision.

Practically all the pathological cases that have been reviewed, tend to support the interpretation of Munk that the kinæsthetic motor arc is not the only one whereby voluntary control is effected.

Lastly, the anatomical facts, the physiological experiments and all the cases of complete kinæsthetic anæsthesia tend to show that voluntary movement, like all other forms of movement, rests upon the existence of a sensory-motor arc; that *without sense processes relevant to the situation*, purely ideational processes, however important they may be as elements in the total mental complex functional in volitional acts, are nevertheless by themselves entirely incompetent to initiate and control such acts. Of all the mental processes that *may* function in consciously controlled acts the *sine qua non* appears to be *sensation*.¹ The facts presented show not only the necessity of centripetal impulses, but the necessity of *sensation*, at least in all forms of voluntary activity not thoroughly practiced and habitual. The relearning under the guidance of vision of lost movements on the part of Munk's monkeys, the cases of Müller, Hoppe, Spiller and Fairbanks, together with the methods used by Goldscheider and others, illustrate this abundantly. Centrifugal processes are of course necessary, but in and of themselves not sufficient. Some of the cases show that even a modicum of sensation from the member to be moved is not sufficient for control and must be supplemented by vision. The fact seems to be that there must be a sufficient amount of sensation to give rise to at least a peripheral consciousness of the member, which so far as the centripetal process is concerned is capable of becoming focal and perceptual at any moment.

IV. The General Theory of Voluntary Movement.

That psychology is in need of a more exactly defined terminology must be evident to every thoughtful reader of its

¹By this term is meant a *conscious, but not necessarily a focal* process.

literature, particularly of that bearing upon movement and the affective processes. There is, however, another sense in which the psychology of movement is in need of a definition. It needs a descriptive and analytical definition of the various kinds and grades of movement as a basis upon which the completer analysis of the processes themselves may proceed. Of course no definition can be *a priori*, but after a certain degree of analysis has been reached it becomes imperative for further work. That the psychology of movement is in need of such a descriptive definition becomes apparent when we consider the processes in terms of which some of the standard discussions describe voluntary action.

Stout tells us that "a volition is a desire qualified and defined by the judgment that, so far as in us lies, we shall bring about the attainment of the desired end." (Voluntary Action, *Mind*, N. S., Vol. 5, p. 356.) He goes on to say, "When attainment is judged impossible, volition in the full sense cannot exist." (*Ibid.*, p. 356.) "In a perfect volition, opposing impulses are not merely held in check; they are driven out of the field. If they continue to exist they do so as external obstacles to a volition already formed. They are no longer motives. They are on the same footing with any other difficulty in the way of attainment." (*Ibid.*, p. 357.) His general position is that "it is the *cognitive* side of our nature which gives determinate character to the conative." (*Ibid.*, p. 356.)

It will be seen that Stout's definitive statements are quite opposite to Wundt's who holds that volitional processes are essentially always *affective* processes (Affecte) (Phys. Psy., 5th ed., Vol. III, p. 244). These feeling processes (Gefühlsvorläufe) are, however, always bound up with a more or less clear sensation or ideational process (*Ibid.*, p. 242). But the primal necessity for a volitional process is an impulsion of some sort or "so-called motive." (*Ibid.*, p. 243.) Still another characteristic of a voluntary act according to Wundt is its duration; it is "never a momentary act," but has a characteristic duration in which there takes place a change of feeling. (*Ibid.*, p. 243.) So nearly alike are volitional and affective processes in Wundt's descriptive definition that in their beginning stages it is impossible to distinguish between affective and volitional activities. The distinction appears only later in the "*Endstadium*" and is to be found in the peculiarly sudden release or change of the affective processes. It must, however, be remembered (and this is important for our own description) that the 'release process' or "*Endstadium*" involves also accompanying ideational processes. (*Ibid.*, p. 245.)

Külpe (Outlines, pp. 265-267) does essentially the same thing, making his brief analysis of voluntary movement fall

within his discussion of the Feelings. He explicitly denies the existence of an "elementary quality" as the distinguishing characteristic of voluntary action and holds that effort is the "one definite phenomenon" which fundamentally characterizes such action. This effort, he goes on to say, "appears to be a complex of more or less vivid organic sensations, composed of tendinous (strain) and articular sensations, peripherally and centrally excited." Külpe dismisses the subject with a brief general description and nowhere tells us in precisely what sense he is using the term voluntary or what the distinguishing marks of the act are, other than that it is markedly the feeling of effort which is a complex of organic sensations. But, as Stout has pointed out, the feeling of effort may "belong as well to the antagonistic tendency which renders the voluntary attitude abortive." (Voluntary Action, *Mind*, N. S., Vol. 5, p. 354.) Moreover the feeling of effort when reduced to the elements of organic sensations is theoretically hardly to be distinguished from emotion when the latter is reduced to its elements, at least according to the prevailing "peripheral" theory. The concept of effort is too vague to be used as the distinguishing mark of voluntary action. Any attempt at complete analysis of the mental processes concerned in voluntary movement is bound to find itself occupied with the cognitive aspects of these processes. It may be urged that this is an objection based upon mere convenience. Even if this were true, it would appear justifiable. Science must everywhere push its analysis in terms of that which most readily lends itself to analysis, provided always, of course, that the analysis does not proceed at the expense of truth and completeness.

The objections urged against the feeling of effort in particular hold also with reference to the feelings in general as distinguishing features in terms of which to analyze and describe our voluntary activities. Against the description of voluntary movement in terms of feeling the following arguments are urged: (1.) Feeling is never the fundamental process in a complex and highly voluntary activity. *Functionally* it is of great importance as a part or aspect of the motive in every voluntary movement from the simple impulse to the volitional act. But, as Titchener has clearly pointed out, the motive is complex, made up of sense or ideational processes on the one hand and affective or feeling processes on the other. But—and this is the important point—the sense or ideational process is always the *conditioning* process, *i. e.*, its nature and meaning always determine the nature and intensity of the feeling.

(2.) Biologically considered the feeling as a psychic process, functional in a controlled response, is entirely too subjective, too vague, too little informational to be regarded as the fun-

damental process in effecting voluntary control. The fundamental process is the one that best reveals the situation to which the response is made. For this reason the term "cognitive" is significantly appropriate to the sense and ideational factors. They reveal the situation and so afford a basis for successful reaction. Organic sensations at best tell us only of the subjective; but voluntary movement, in the last analysis, finds its cues and control factors (not necessarily its entire motivation) in external conditions. One might even go so far as to say that every movement that takes place wholly on the basis of the subjective and without any regard to objective or extrabodily conditions of a sensory or ideational nature is essentially of the reflex or automatic type.

(3.) Common observation teaches also that the predominance of purely subjective elements is always an interference in volitional performances. Feeling doubtless plays a highly important function in the *initiation* of many voluntary activities but the position taken here is that *voluntary movement is not a matter of initiation of movement nor a peculiar species of movement per se, but is essentially a form of control and in this respect alone differs primarily from other kinds of movement.* So far then as voluntary acts are concerned, the initiation of movement may be assumed; it is provided by the physical and mental constitution of the organism which, in the psychology of the individual, must be assumed. Biologically voluntary movement is a peculiarly *individual* form of adaptive control. Perhaps it would be better to say that it is peculiarly individual just because it is adaptive to conditions that are non-racial and therefore individual. Psychologically voluntary movement is a kind of movement in which sensory or ideational processes are *necessarily* functionally present. All non-voluntary movements are therefore to be defined in biological or physiological terms; in them sensory or ideational processes are unnecessary for their efficient execution. Practiced and habitual movements, or such as are only partially racial, are, on the other hand, movements in which psychical processes are still required at critical points. Many of our instincts and practically all of our common habits belong to this grade. But whenever an instinct or a habit is for any reason lifted temporarily to the higher volitional level, it is always with an increase in clearness and definiteness of the accompanying *cognitive* processes that this is accomplished. In case of the instincts the increased clearness, together with perhaps an increased complexity, of the cognitive elements is precisely what makes the instinct a voluntary activity. Or, to state it in another way, the instinct becomes voluntary directly as the cognitive processes gain in clearness.

We should say, therefore, that instead of voluntary movement being a feeling modified by a judgment, it is rather a complex process in which a feeling reinforces the motor aspect of the focal or cognitive elements. The position here taken is that, whatever may be true in other forms of conscious states, for voluntary movement so long as feeling predominates consciousness, that is so long as the cognitive elements are submerged or "swamped"—to use Titchener's phrase—there is no *voluntary* movement; that voluntary movement can begin only with the appearance of distinct cognition or at least distinct sensation, and that with the "swamping" of the latter there goes also the disappearance of voluntary movement. It is contended that *voluntary movement means control and that control in turn means cognition.*

(4.) To this interpretation the physiological experiments of Mott and Sherrington and Munk together with the pathological cases already reviewed lend strong support. They tend to show that all movement above the reflex level requires *sensation* for its execution. They tend to show, too, that a feeling, desire to move, or *Gemüthebewegung*, without the cognitive process, is entirely incapable of controlling the movement. Certain of the cases of anæsthesia show that there are conditions when "inner" volition, or desire to make a movement, is entirely possible without the ability to make the movement, *not because it is physiologically impossible but because it is psychically impossible* for the reason that indispensable cognitive data are wanting. It follows therefore that all movement taking place while feeling is dominating consciousness is of the reflex-instinctive type; that under such conscious conditions voluntary movement is only partially, and perhaps not at all, possible; that control (the essence of voluntary movement), demands at least two things, awareness of the position of the member to be moved and consciousness of the current results of the movement.

(5.) Once more, voluntary movement necessitates voluntary or controlled attention. In fact, so close is the relation between voluntary movement and attention that if one neglects the *nature* of the processes, voluntary movement may be accounted for wholly in terms of attention. But upon what material does attention naturally operate, upon the feelings or upon the sensory and ideational processes? By what may perhaps be properly called a process of selection attention goes to those processes which by being rendered focal and clear will most facilitate control. Here, again, we have evidence for the primacy of the cognitive processes.

Voluntary movement is peculiarly psychical and therefore as a *kind* of movement almost wholly a psychological prob-

lem. Physiologically it is not a new form of movement bringing into play necessarily a new coördination. Looked at as an activity of a muscular mechanism it is highly probable that new coördinations never take rise in voluntary movement. What is new is the making of these muscular coördinations in response to psychical conditions rather than in response to purely neural and physiological conditions. In short, it is a new *control* rather than a new muscular coördination; or, to put the matter in another way, it is a new chaining together of coördinations which in themselves are old. The reflexes and the instincts supply all the neuro-muscular pathways. When these pathways are utilized under individual conditions, instead of old and racial conditions, we have voluntary movement.

It follows from what has been said that all movement as such begins as a purely physiological process; that, at least so far as the individual is concerned, the reflex or impulse is the prototype of all action. This appears so evident as to call for no detailed discussion.

A second consequence of the view here presented is that the earliest movement-consciousness is in no sense the cause of the movement, but only a by-product of a complex psychophysical mechanism, the physical aspect of which becomes operative before the mental. Experimental evidence for this is found in the studies of Bair and Woodworth, who demonstrated that there is consciousness of a single movement taking place in a larger complex movement before there is ability to control that movement in isolation. We are at times apparently conscious of our reflexes without at the same time being conscious of any control over them whatsoever. This is particularly true of instinctive behavior of the reflex type. In fact it is very probable that at any moment our conscious life is wider than its functional aspects, that in the most voluntary activities the functional processes are never coextensive and identical with the conscious processes. The functional processes never make their first appearance as such, but appear as epiphenomena and then, after they are picked out of the total conscious complex by attention, become controlling factors. In support of this point also the work of Bair affords evidence. The learning of complicated activities, in so far as they are consciously, or voluntarily, learned, like riding a bicycle, etc., illustrates the same general truth. The correct movement is reflexly made, or accidentally hit upon, under guidance of the general intention, as a consequence of excess stimulation. Certain sense elements connected with this movement are then caught by attention. These elements then become functional by virtue of the effect of attention. The

supporter of the kinæsthetic theory may contend that it is impossible to conceive how control can *begin*, except the results of the movement previously made in a reflex manner are first *imaged*. This is the position taken by James when he says: "In the chapter on the Will we shall learn that movements themselves are results of images coming before the mind, images sometimes of feelings in the moving part, sometimes of the movement's effects on eye and ear, and sometimes (if the movement be originally reflex or instinctive), of its natural stimulus or exciting cause." (Prin. of Psy., Vol. I, p. 445.) Our own position is a refinement of that set forth by James. We contend, first, that the perception or image "of its natural stimulus or exciting cause" is always sufficient to *initiate* the movement once it has been learned or practiced and that the *control* is effected by *immediate* sensations and perceptions and not by memory images of previous results. We contend further that in the period of learning or practice the process is not one in which memory images "resident or remote" play the primary rôle, but that it is on the other hand primarily a process of varied inhibition and re-enforcement, based on the perception of results, this perception being either of a kinæsthetic nature or of a visual or auditory nature, depending upon the nature of the movement and probably also upon the hereditary and acquired nature of the individual.

At this point the function of feeling in voluntary movement is apparent. The inhibition and re-enforcement are in each instance very largely due to the feeling aroused by the perception of the result. Let it be observed, however, that in each case the nature of the feeling is determined by the nature of the cognitive process.

While the purely ideational processes are of considerable importance in initiating movement and of slight importance in its control the conditions with regard to the sensory processes are reversed. Movements initiated by purely sensory processes are rarely if ever voluntary. On the other hand movements originally initiated by purely ideational processes are rarely, if ever, other than voluntary. This is only natural. Ideational processes are racially recent and novel; they are not provided with a mechanism of preformed pathways by means of which expression and control quickly become coördinated and exact. "All consciousness is motor," but not equally so, nor with equal definiteness of expression. The sensory processes are both more motor in tendency and more definite in reaction. Ideational processes, so to speak, lack a mechanism of their own. As elements in movement they have come into an organism built upon a sensory-motor basis. They have come into what Morgan has called a "going concern" and therefore as factors

in voluntary action must make use of a mechanism that is essentially sensory-motor and non-voluntary. So far then as the initiation of voluntary movement is concerned, this means a readaptation of the mechanism for ideational ends. Ideational processes are therefore necessarily present in the original initiation of voluntary movement.

So long as a purely sensory content is sufficient as a cue to adaptive movement, just so long there is a preformed sensory-motor mechanism provided for the execution of the corresponding movement. In short, there is a mechanical or "uncontrolled" response only when there is a mechanical reception of the stimulus or situation. That is, given conscious meaning in the *psychical* response, there is bound to be *control* in the *movement* response. But conscious meaning always implies the presence of ideational processes. We therefore conclude that whenever ideational processes as such are the initiatory processes there is always some control in the resulting movement. This means that ideational processes are never epiphenomenal in a movement-consciousness, provided, of course, they are related to the movement. An ideational process cannot directly initiate a movement without the movement being a controlled one, at least in its earliest stage. The presence of an ideational process means a higher level of consciousness, which in passing over into bodily activity carries with it a consciousness of the control that invariably becomes an element in the control. We cannot *think* about our movements while they are taking place without in a measure controlling them from this high-level consciousness which, whenever present, is always *functionally* present. We cannot ideationally cause a wink of the eye without to a degree at least controlling the resulting wink. We cannot think about our walking while we walk without to a degree controlling it.

This does not mean that all consciously controlled movement is *originally* initiated by an ideational process. It has already been pointed out that all sorts of processes may serve as starting points for the reason that "all consciousness is motor." It means only that when an ideational process *does* start a movement the resulting movement is, at least in its initial stages, more or less consciously controlled. But if a movement started as a non-volitional process, becomes a controlled activity, *it always becomes such in consequence of some sort of ideational process*. That is, so long as the initial sensorial process remains such (without meaning), there is no voluntary control. Control is never initiated except there be a psychical reason for it, and such "reason" cannot be found in a purely sensory process devoid of meaning. In other

words, the motive that leads to the initiation of voluntary control of a process capable of sustaining itself without such control must contain meaning, which is a psychic element of the higher level.

Nor is this meant to imply that the control as such is essentially carried on by ideational processes, however important these may be in bringing about the control. In matters of control ideational processes are only vicarious and supplementary, and even then their functional value may be questioned. Control is necessarily perceptual and sensory.

The theory of voluntary movement that has been here presented starts from a physiological process, and ends again with a physiological process, making psychic processes and attention middle terms in effecting a readjustment to the environment. This theory holds that the first control in learning a new movement is always gained by attention picking out certain elements, which in their first appearance are entirely non-functional.

It holds, too, that throughout the volitional or highest grade of movement, both clear imagery and clear perception are necessary for proper initiation and efficient control. It holds further that in the lower or practiced grade, in which attention leaves the process, it may do so by way of the substitution of a general symbol for the details of the original process to which attention was necessarily given at the beginning of the practice.

The author desires to express here his appreciation of the criticism given to this paper at every stage of its preparation by Dr. E. C. Sanford. While he is not to be held responsible for the facts cited or positions taken, his generous and stimulating criticism has been of inestimable help.

AN EXPERIMENTAL STUDY OF BELIEF¹

By T. OKABE, PH.D.

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The object of the present study is to describe, in analytical terms, the experience of Belief, as it appeared under experimental conditions. Although a good deal has been written, by psychologists, upon the subject of Belief, it is not too much to say that the status of the experience is still very uncertain, and its place in the system of psychology undetermined. A brief historical review will suffice for the support of this statement.

Perhaps the first writer to give an empirical psychological criterion of Belief is David Hume. Belief or Assent is the vivacity of the perceptions presented by memory and the senses, the force and liveliness of these perceptions; it is a strong and steady conception of any idea (*A Treatise of Human Nature*, ed. by L. A. Selby-Bigge, 1888, 86, 97 n, 101, 103, 116, 119); it is the manner of our conceiving ideas which bestows on them an additional force and vivacity (96). An Opinion or Belief is a lively idea related to or associated with a present impression (96, 93, 97, 98, 209, 626); the relation is due to custom (102, 626), and the Belief arises only from causation (107; cf. 113). The vivacity proceeding from a customary conjunction with a present impression is an ultimate character of ideas (636, 629); that of the historical narration, *e. g.*, is more forcible and real than that of poetry (631).

According to James Mill, Belief is an inseparable association. "In every instance of Belief, there is indissoluble association of the ideas . . . Cases of indissoluble association, admitted by all men to be this, and nothing more, are acknowledged as Belief" (*Analysis of the Phenomena of the Human Mind*, i., 1869, 368 f.). Here is the

¹ From the Psychological Laboratory of Cornell University.

one side of the Humian doctrine. According to John Stuart Mill, Belief is more than an inseparable association (*ibid.*, 418). "What is the difference to *our minds* between thinking of a reality, and representing to ourselves an imaginary picture? I confess that I can perceive no escape from the opinion that the distinction is ultimate and primordial. There is no more difficulty in holding it to be so, than in holding the difference between a sensation and an idea to be primordial. It seems almost another aspect of the same difference. . . . Grant these differences, and there is nothing further to explain in the phenomenon of Belief" (412 f., 419 ff.). Here is the other side of the Humian doctrine, though its statement is less concrete than in the pages of Hume himself.

Alexander Bain finds "the first germ and perennial substance" of Belief in what he terms primitive credulity: "we begin by believing everything; whatever is, is true." "We may, if we please, call it an impotence of thought; for, without some positive interference from without, there is no other way of doing or thinking." Belief as a specific experience "is distinguished when we suffer the shock of a contradiction, a check or disappointment in some career of activity." This Belief, an "innate credulity tempered by checks," is influenced by various factors, intellectual, emotional and active. Logically, it is opposed to disbelief; "but as a mental fact these two states are identical . . . The real opposite of Belief as a state of mind is not disbelief, but doubt, uncertainty." What precisely "the state of mind called Belief, expectation, confidence, trust, assurance, conviction" is, in psychological terms, Bain does not tell us; it involves intellect, but knowledge is not the whole of it; it is often accompanied by strong emotion, yet emotion does not amount to believing. Since "preparedness to act upon what we affirm is admitted on all hands to be the sole, the genuine, the unmistakable criterion of Belief," it is clear that Belief is a mode of will; it has its germ, as we have seen, in the primitive tendency to accept whatever has not yet been contradicted; at the same time, the working of this primordial impulse would not of itself have led to any mention or differentiation of Belief; there is a necessary intellectual element,—some cognisance of the order of nature, or of the course of the world. In a word, Bain gives us a genetic account of Belief, tracing its sources, constituents and dependences in general terms; but he does not offer us an analysis of the actual experience of Belief (*The Emotions and the Will*, 1880, 505 ff.). The most important features of this account are, first, the insistence on the naturalness or nativeness of Belief, the mind's instinctive tendency to accept, so that the thing to be explained in psychology is rather doubt or obstructed belief than Belief itself; and, secondly, the couching of a theory of Belief in 'motor' terms. These two features do not appear to be necessarily connected.

Herbert Spencer follows James Mill in reducing Belief to an indissoluble association (*Principles of Psychology*, i., 1881, 421; ii., 406 ff., 505*hh-ii*). Franz Brentano, on the other hand, accepts and recasts the doctrine of John Mill. "Nach unserer Ausdrucksweise ist seine Lehre die, dass Vorstellen und Urtheilen zwei völlig verschiedene Arten der Beziehung auf einen Inhalt, zwei grundverschiedene Weisen des Bewusstseins von einem Gegenstande seien" (*Psychologie vom empirischen Standpunkte*, i., 1874, 275). Neither of these authors helps us towards a psychological analysis.

So far we have found Belief described as an elementary process or function of mind, and as a complex intellectual or volitional formation. The affective interpretation has not either been wanting. It

has, indeed, been ascribed to Hume himself, on the basis of the following passage: "The difference between fiction and Belief lies in some sentiment or feeling, which is annexed to the latter, not to the former" (*An Enquiry Concerning Human Understanding*, Religion of Science Library, 1900, 48). It is clear, however, from other passages, that Hume's doctrine of Belief in the *Enquiry* is in substance identical with that of the *Treatise*: what characterises Belief is not so much a 'feeling' as a 'feeling' (48 ff., 112; cf. W. B. Elkin, *Hume*, 1904, 172 ff.). W. James, on the other hand, declares that "in its inner nature Belief, or the sense of reality, is a sort of feeling more allied to the emotions than to anything else;" "Belief consists in an emotional reaction of the entire man on an object." At the same time, he thinks that it resists further analysis: "Belief, the sense of reality, feels like itself—that is about as much as we can say;" "this attitude is a state of consciousness *sui generis*, about which nothing more can be said in the way of internal analysis." James agrees with Bain that the true opposites of Belief, psychologically considered, are doubt and inquiry, not disbelief, and that the primitive impulse is to affirm immediately the reality of all that is conceived. He agrees, also, that Belief is closely connected with motor activity: "motor effects are apt to follow;" "consent and Belief are both intimately connected with subsequent practical activity" (*Principles of Psychology*, ii., 1890, 283 ff.). We need not here follow James into his discussion of the conditions of Belief. We note that W. Bagehot speaks explicitly of Belief as an emotion, the emotion of conviction (*Literary Studies*, i., 1879, 412 ff.).

Three other modern psychologists—J. Sully, J. M. Baldwin, G. F. Stout—call for mention in this connection. According to Sully, Belief is a compound of three factors: intellectual representation, feeling, and active impulse. Intellectually regarded, Belief "marks off the objective attitude of ideation or thought, or, in other words, the fact of its representativeness." Affectively regarded, Belief is "the state or feeling of restful assurance, which is always present in some degree when we judge or decide upon a matter, and which gives to our judgment its characteristic psychical coloring." Again, Belief stands in organic connection with action; Belief and activity react the one upon the other. "In most cases, at any rate, Belief involves the incipient excitation of impulses to look out for a result, and to follow a line of action" (*The Human Mind*, i., 1892, 453 f., 483 ff.; ii., 276 ff.; *Sensation and Intuition*, 1880, 75 ff.).

Belief, as a specific mental state or process, is treated by Baldwin in his *Handbook of Psychology*, ii., 1891, *Feeling and Will*. "The feeling of Belief is a feeling which attaches to the representative faculty primarily." "It is a feeling of confirmation and security over and above the feeling of simple reality;" "Belief in anything is the consciousness of the presence of that thing as fitted to satisfy a need; and it is distinguished from the earlier unreflecting reality-feeling, which is the simple consciousness of a presence;" Belief is "a feeling arising from the successful outgo of impulse." Baldwin regards this feeling as *sui generis*: "as a feeling, Belief cannot be explained any more than any other feeling; it must be felt; further remarks are really upon the physiological and psychical conditions under which this feeling arises" (155 ff.).

Stout uses the term Belief in a wide sense as interchangeable with judgment, and regards it as an attitude of consciousness towards its object, fundamentally distinct from simple apprehension; in this position he follows Brentano (*Analytic Psychology*, i., 1896, 96 ff., 115; ii., 234 ff.).

In Titchener's *Outline of Psychology*, 1896, 315 f., Belief is given a place among the intellectual sentiments, and a method of investigation is proposed. The passage runs as follows:

"The intellectual sentiments might be investigated in the following way. Prepare a number of reasoned statements—or select them from the lists given in the text-books of formal logic—some of which are correct, while others contain various logical fallacies. Let the subject give a careful introspective account of the 'feelings' aroused by their reading. . . . A rough notion of the number and forms of the intellectual sentiments can be obtained by introspection of consciousness during the reading of a piece of scientific reasoning, or the hearing of a scientific lecture."

Here is evidently a suggestion of the experimental method which has recently been widely used in the investigation of the processes of thought. We ourselves employed the method in two forms: as a method of single exposures, and as a method of paired comparisons. We prepared a long list of sentences, with a view to the arousal, in the mind of the observer, of belief, disbelief or doubt; the sentences were made as short as possible, in order to avoid unnecessary complications in understanding, and were selected with an eye to the interests and attainments of the observers. Series were also taken in which the sentences were replaced by mathematical expressions. These four principal experiments will be reported separately.

A long series of preliminary observations was made by Professors Titchener and Bentley. Sentences, calculated to arouse belief or disbelief, were typewritten at the head of sheets of paper, and the observers were instructed to read with understanding, to note the appearance of belief or disbelief, and if either of these consciousnesses occurred to write a full introspective account of the experience. No limit of time was set for the understanding, and no record was taken of the time required for the appearance of belief or disbelief; the object of the observations was a purely qualitative analysis. The tests showed, in general, that there is a definite consciousness which is unhesitatingly recognised and named as belief or disbelief, and is distinguished from mere assent, or passive acceptance, but that this consciousness is of comparatively rare occurrence. It was surprising, both to the experimenter and to the observers, that sentences which seemed likely to arouse a strong response, of belief or disbelief, were taken coolly and passively; the observers were 'ready' to believe, knew that the analysis of belief was the 'problem' before them, and nevertheless found, in the great majority of cases, that the sentences were accepted or dismissed as if automatically, or that they were met with an indifferent suspense of judgment. We may say at once that a like difficulty,

though of less degree, was encountered in all the regular observations. It may, no doubt, be ascribed in good part to the nature of the stimuli employed,—detached sentences, laid before the observers apart from any context or cumulative argument. At the same time, if casual but long-continued observation by the experimenter and others may be trusted, a distinctive belief-consciousness is not of common occurrence in everyday life. This statement, however, necessarily remains vague until we have given a detailed analysis of that consciousness.

METHOD OF SINGLE EXPOSURES: SENTENCES, VISUAL

In this first series of experiments, the observer, seated comfortably at a table, was shown a typewritten statement, and was instructed to read it with concentrated attention, to note whether it aroused belief or disbelief, and on the appearance of either consciousness to close his eyes and to dictate to the experimenter a full account of the experience. In a certain number of cases the introspective record was fractionated: that is, in some observations the period covered extended only to the understanding of the sentence, in others it began with the understanding and ended with a clearly formed belief or disbelief; we have not thought it necessary, however, to give these fractionated results a separate treatment. No time records were taken. The regular observers were Miss A. de Vries (V), a graduate student in psychology; Miss H. Clarke (C), fellow in psychology; Dr. L. R. Geissler (G), instructor in psychology; and Mr. W. S. Foster (F), assistant in psychology in Cornell University. Less numerous observations were made by two other students, Mrs. Piotrowska (P) and Mr. Clarke (Cl).

It should be said that all four of the regular observers had taken part, in the previous year, in a Seminary discussion of the experimental investigation of the thought-processes. They were therefore familiar with the method employed, with its limitations and dangers, and were especially alive to the difference between informatory report and psychological description. Largely for this reason, questioning by the experimenter was ordinarily unnecessary, and was in fact very sparingly used. Recourse was had to it only when the observer was obviously reporting in verbal shorthand, and even then it was made as little suggestive as possible; in particular, no hint was given that certain processes were wanted or expected by the experimenter, and no limit was set to the observer's vocabulary. There was, of course, a good deal of *Kundgabe* in the records at large, and we were content to let it stand as such; only as regards the consciousness of Belief

proper were we at great pains to translate the *Kundgabe*, where it occurred, into *Beschreibung*. Belief and disbelief were, as has been said, the strict topic of our analytical inquiry.

In order to deal effectively with the great bulk of introspective material, we take up the observers in order, give a brief characterisation of their mental constitution, and then state the results of their analysis. Selections from the introspective records are appended as evidence. The numbers prefixed to the general statements correspond to those prefixed, later, to the selected introspections. Samples of the sentences employed will be found at the end of this paper.

Observer V

With this observer we made 196 successful tests. *V* is of an impartially imaginal type, (1) and is especially liable to empathic kinæsthesia, sensory and imaginal. It is interesting, as an item of individual psychology, that verbal images were of comparatively rare occurrence, and that they occurred always with disbelief, never with belief. Belief and disbelief are characterised by a fairly intensive affective process, (2) which is almost always pleasant; the pleasantness appears as well with disbelief as with belief. The most important and characteristic factor in the belief-disbelief consciousness was an organic (mainly kinæsthetic) complex, which stood out clearly and sharply from the other contents of the moment. (3a) Slowing and deepening of breathing, with attendant change of sensation, were frequently reported. There were also (3b) kinæsthetic sensations from a shift of bodily attitude, from sitting up and straightening in the chair. With strong belief or disbelief there was (3c) a 'feeling of power' or 'feeling of superiority;' the whole body felt as if stiffened into something massive and strong.

V had a personal interest in the experiments, and took the 'problem' very seriously. Possibly for this reason she showed a type of reaction which was not found with the other observers; (4) as soon as the sentence was read, there was a belief-disbelief consciousness of an indefinite kind; then the sentence was re-read, with a view to complete understanding, or to verification of the previous understanding; and then, finally, belief-disbelief emerged in definite form, and in stronger or weaker degree as the case might be. When the decision had been taken, and belief or disbelief established, (5) the aggressive attitude changed to a relaxation of muscular strain, with the emotion of relief.

(1) A 131. Imaginary image of a Chinese in the water. Felt choked, as if I were drowning. A 126. Visual image of a trick ele-

phant dancing. Felt big and clumsy myself, as if I were the elephant.

(2) A 59. Decided I did not believe it. Pleasant. A 81. There was no hesitation about understanding it and coming to this conclusion. Pleasant. A 58. Rejected the whole statement as unsafe. Don't believe it. Decisive. Pleasant. A 125. The feeling of disbelief was definite, clear, like a streak of yellow color or the call of a trumpet. Felt pleasant. A 60. Decided in an instant that I disbelieved it. Pleasant. A 102. Verbal saying over to myself: Of course don't believe it. Satisfaction. A 11. The feeling that I was right in not believing it . . . was pleasant. A 88. Clear verbal ideas: There's something wrong; it isn't a good comparison. That was pleasant.

(3a) A 3. Believed it. . . . Sensations from deep breathing. A 10. Sensations from slow breathing. . . . Believed it. A 125. Felt strong, and sensations from deep breathing. (Disbelief.) A 58. Sensation from sitting up, and long breathing. (Disbelief.)

(3b) A 60. Straightened up. (Disbelief.) A 11. Sensations from frowning and sitting up; felt tightening of jaw and strain of arms. Disagreed with everything in the statement. A 17. Sensations from straightening up, and I knew I did not believe it. A 56. Belief strengthened the more I read it. Felt sure I was right. Sensations from sitting up straight.

(3c) A 11. Felt aggressive. (Disbelief.) A 58, A 125, etc. Felt strong. A 12. Knew I believed it. Felt satisfied and rather superior, as if my decision were surely right. A 29. Belief came suddenly and was certain. Felt I was right in believing it. Pleasant, and felt strong and cool. A 32. Believed this part. Pleasant, and warm sensations up and down the spine. Rejected all the rest as a mass of useless words. Felt superior.

(4) A 77. Read and understood; did not believe it. [Then follows a stage of thought, given mainly in attitudinal terms.] Then I knew why I disbelieved the sentence, and felt satisfied. A 7. Read sentence and had clear auditory after-images. Decided at once that I believed it. This was very pleasant. [Then follows a stage of doubt and deliberation.] Decided I had been right in believing it. Felt pleasant. A 24. Read it once and knew I would never believe it. [Then follows a stage of consideration.] Felt superior; sensations of straightening up and of deep breathing; sensations also from skin of face around the mouth. Then rejected the whole thing at once.

(5) A 7. Muscles all relaxed. A 21. Felt relaxation when I decided. A 60. Felt relief. A 1. This I do believe, and when this was decided I relaxed. A 10. Relaxed all over. A 11. At the end, my muscles were all relaxed and I sighed in relief. A 12. As I became sure I had kinæsthetic sensations from nodding, and relaxed.

Observer C

With this observer we made 218 successful tests. C is an excellent visualiser, and less impartial in type than V. (1) Almost all her reports contained descriptions of visual imagery, sometimes vague and schematic, sometimes sharp and clear. (2) The reaction of belief-disbelief was usually affective; but the affective processes were less intensive than for V, and were often qualified by a 'rather' or 'slightly.' With very few exceptions, the experience of belief was pleasant, that of disbelief

unpleasant. (3) Organic sensations were seldom reported, and were not intensive. (4) The most important and characteristic factor in the belief-disbelief consciousness was a 'perception' of agreement or disagreement, of harmony or disharmony, which was difficult to analyse, but seemed in every case to be preponderantly visual. (5) This 'perception' was clinched and expressed in verbal form; with belief came the verbal ideas 'Yes,' 'That is true,' etc., and with disbelief the verbal ideas 'No,' 'It isn't true,' etc. Internal speech, overt or abbreviated, was very common.

(1) A 59. Meaning was perfectly clear and was illustrated at every step by what might be called symbolic visual images. The whole thought was thus objectified. E 1. The word natural was attended by the visual image of the outside of Max Müller's book on Natural Religion. . . . In connection with first part of sentence, visual image of a savage on the seashore. . . . There was also a schematic representation of a savage as related to other members of his society. This was mostly a matter of geometrical lines with the savage himself in an angle, and was too vague to be definitely described. E 2. Very clear visual images of the whole incident described. Saw the child in the schoolroom, even having a vague idea of his place in the room, and followed him to the store which was definitely located in reference to the room. . . . There was also an image of the man who answered at the other end of the telephone. A 129. Visual image of a page of Spencer's Data of Ethics with the paragraph marks which he uses. E 9. An imaginative image of the infant being brought into the room and the change of expression in his face. Then memory images of babies I have known, especially one. . . . The image of the real baby changed somewhat to agree with my memory of him at different times, and was set upon a sort of dissolving background, now his own home, now a room in his grandmother's house. The latter was quite distinct and definite. There was a background to the imaginative images too, a room, part of which I could describe rather definitely. Here, however, there was no tendency to change; the image was about as static as a visual perception and remains so yet.

(2) A 11. Assent was accompanied by a feeling of pleasure. A 7. The assent in this case was accompanied by an agreeable feeling, what might be called a feeling of satisfaction. A 12. The assent was accompanied by a pleasurable feeling and amusement. A 56. I assented to the proposition and the feeling was slightly agreeable. E 3. Disbelief. . . . Attitude of dissatisfaction.

(3) E 3. Negation at every step accompanied by unpleasantly unsettled organic sensations. A 59. Feeling of strain in the eyes and holding the breath. [Reports of this degree of definiteness were very rare.]

(4) E 9. Conscious attitude of lack of agreement with my experience. I don't know what this was. It was a certain visual blankness in part; I found no corresponding fact in my experience. E 6. Visual images of Rousseau as I have always imagined him, not as he looks in his picture. Visual image of several people in a wood, representing my idea of Rousseau's return to nature. These were both imaginative images originally, but have long represented these ideas to me, and are in a sense memory images now. I was aware that the statement agreed with what I knew of Rousseau. It is hard to tell what this agreement was; it seemed to lie in the images. E 5. Visual image of

the outside of a book that I once read on a related subject. Schematic representation of opinions that I have read or held about the question. I seemed to see a long and broad trench about as wide as a street, and the opinions were in some way localised there. A consciousness of agreement between them and the sentence; this seemed to belong to the visual representation of them. E 11. Visual image of one of Max Müller's books with his name on the outside. A series of visual images that represented his theory to me when I first read it. Feeling of amusement at the incongruity of the statement just read with the general trend of Max Müller's theories. Then a very distinct image of people inventing language through action, as I perceived it when read. Idea, vaguely verbal, that there was less contradiction here. There was more in this than words. I think the two ideas seemed spatially closer than in the other case, though only one of them was represented by a distinct image.

(5) E 1. Verbal image: This is only relative. E 2. Verbal image: It may be true. E 4. Verbal image: Imageless thought. E 7. Verbal image: Of course a deaf-mute has concepts. A 12. I said: Yes, especially from the standpoint of structural psychology. E 3. Verbal image: Not every one. E 9. Verbal image: May be true; I don't know. A 129. Verbal images: Believe Spencer said it; not so sure it's true. A 22. Said: I don't know; and then: Probable. A 59. Said the word Analogy. . . . Gave a partial assent in the words: It is probable.

Observer G

With this observer we made 170 successful tests. *G* may be characterised as of the verbal-imageless type; in his ordinary thinking he makes no use of visual images, and though he is a skilled musician he has no mental furniture of auditory images; he thinks, if the phrase may be coined, in verbal awarenesses. Our own experiments, as well as other and similar experiments in which he has recently taken part, prove that he has, at least in some degree, the capacity for images, visual and auditory; "the assimilation of a new idea, or the understanding of a novel term, is for *G* a definitely imaginal experience, but with growing familiarity the images very quickly lapse."¹ We find, in fact, (1) that *G* occasionally reports imagery, though in this matter his introspections, as a whole, are in marked contrast to those of *V* and *C*. Almost invariably (2) he reports internal speech: sometimes in the form of properly formulated sentences, usually in what *G* terms 'shorthand,' a more or less abbreviated and schematic representation.² The verbal images Yes, No, and Nonsense were the commonest. (3) Organic sensations are not a marked feature of *G*'s consciousness. (4)

¹Titchener: *Experimental Psychology of the Thought-processes*, 1909, 248.

²The verbal shorthand consisted, in general, of an intermittent pressure of the tongue against the palate, and of the setting of the organs of speech for the articulation of initial consonants. *G* will publish, in another place, a full introspective account of this mode of verbal ideation.

He is, throughout this series of experiments, reluctant to identify belief either with verbal imagery or with kinæsthesia; he seeks to give it a formal explanation, as an Herbartian apperception, or a Wundtian assimilation; he speaks of the 'fusion' of the statement with memory ideas, and of a 'conflict of meanings'; yet he fails to find any conscious contents aside from the words and the kinæsthesia; belief is beaten up with these experiences, as in the case of *C* it is with the visual images. (5) The affective side of the experience is fairly strong; with a few exceptions, which can be explained, belief is pleasant and disbelief unpleasant. While, as we have said, belief was not attended by any pronounced kinæsthetic or other organic complex, the presence of muscular tension is shown (6) by the frequent report of an emotion of relief after assent has been given or withheld.

(1) A 5. At the same time vague memory idea, illustrating this principle, of an automobile localised in vague visual terms in front of the Armory, where I frequently have seen them backing up or trying to turn round. I don't know why I should call these images visual, because I don't see anything, but somehow, whether by habit or imitation, I refer them to some strain in the muscles of the eyeball. Anyhow, the images are so vague that they might just as well be entirely kinæsthetic.¹ A 126. Vague visual image, when I closed my eyes, of a gray mass, vaguely of the shape of an elephant, and then another visual image, a triangular shaped, whitish image corresponding to a circus tent. A 90. Visually, in very vague images, I saw myself in the Acoustics Room, where all my experiments have been done. A 9. The word 'expression' was accompanied by a visual image of a picture of an actress, seen in my room just before I left the house. A 87. I had vague image of Professor Titchener in the Seminary Room and of his situation, and vague auditory image of his voice. A 10. Very vague and indefinite picture of battle with soldiers charging and getting wounded. Now attention seemed directed to man lying wounded in the foreground. . . Now a man slightly wounded arose and began to attack.

(2) A 114. My reaction towards this statement was mostly in verbal form; I had a strong muscular tendency to say aloud: Oh, that is nonsense. The words: Oh, nonsense, are one of my habitual reactions to statements that strike me as absurd. A 116. In more or less abbreviated verbal form the question arose in me: How can movement direct the trend of thought? . . . The words: strain, attitude, kinæsthetics, sounded in my ears. A 58. The refusal to accept was accompanied by motor tendency to habitual expression: Aha! A 101. While visually reading there was verbal comment in the background. This consisted in slight innervations of the throat and back of the mouth for the expressions What is that? That is absurd, Nonsense, No. A 77. Had abbreviated verbal comment, which if written out would be: Intelligent persons are not always white men, and more pressure does not mean more power. A 126. That is disbelief, because verbal comment ran: Implies reasoning in animals, which doesn't exist. A 84. Brentano's name came up (internal speech).

¹ Cf. the analysis of the memory image given by C. W. Perky, this *Journal*, xxi. 1910, 450.

. . . At once the thought struck me: This is a very empty phrase if you take it literally, and there is no danger of getting into trouble if you accept it: this thought was not so elaborate in consciousness, but occurred in the usual shorthand verbal fashion.—Reports of this sort are as common as those of the former sort are rare.

(3) A 101. Reread the whole statement and had a slight kinæsthetic tendency to shake my head. A 58. Empty, blank consciousness . . . was simply absence of ideas, while a slight organic or kinæsthetic background was present.—More than once *G* reports 'a tendency to smile and say: That is nonsense; the tendency to smile was mostly kinæsthetic or motor tension in the face, and especially around the eyes.' Often the report is given: 'I did not notice any kinæsthetic or organic processes.'

(4) A 3. As soon as the whole phrase fused or associated with the idea that this means, in other words, that the whole world is a total mental process, then agreement was established with a strongly pleasant relief noticeable also in the body and in change of position. In this case the attitude was markedly kinæsthetic, and not a mere association of ideas. A 78. When I got as far as the word Kicking (in reading the first time) my left foot (the left leg being crossed over the right) started more or less unconsciously a quite marked upward movement and caused a host of kinæsthetic strain and muscular sensations. This concrete example (whether merely coincidence or not) or illustration of the statement made the acceptance of its truth easy and pleasant, although as a rule I am very sceptical with regard to motor tendencies. But this concrete example convinced me quite strongly that the state of acceptance or belief is mostly a fusion of new ideas with memory ideas. In this particular case it was a fusion (or coincidence) of a new idea with a homogeneous perception. By homogeneous I mean a perception of the same content as the ideated content. A 79. Disbelief, because there is a host of verbal objections, verbally more or less articulated objections. I don't know how to describe it otherwise than as a conflict of meanings. These conflicting meanings arise already during reading, and become focal after I get through reading. I can't analyse these meanings. They attach themselves to verbal articulations. The conflict itself can't either be analysed; it is too complicated. A 19. Disbelief came especially in the word: Meek. There was a conflict. . . Conflict in meaning arises in verbal ideas which have opposite meanings, whatever that is; for instance, Meek suggests Strong, and I prefer Strong to Meek.

Once the conflict is given in affective terms. A 84. There seemed to be a quick change from slight unpleasantness (going with the meaninglessness of the statement) to slight pleasantness in accepting it without risking anything.—Several times reference is made to a quick alternation of exemplifying ideas, given in verbal terms. 'These ideas were very vague and flash-like and quick, with almost no content.'

Sometimes the absence of conflict appears to condition belief. A 9. No images to the contrary arose, so the sentence was accepted silently, with a slightly pleasant feeling. A 21. Acceptance seemed to consist in the absence of any conflict between the statement and associative ideas. I cannot describe what the conflict would be like.

The 'fusion of new ideas with memory ideas' appears in many reports from this observer. A 90. There was a slight tendency to accept the statement on account of the ease with which memory ideas occur and fuse with the statement as read. I cannot say what is the form of the memory ideas; just a background of attitude. A 87. I am somewhat doubtful of the last phrase, because I haven't images

enough to illustrate my understanding of it. In other words, the last phrase does not call up images of any kind, with which it could fuse. [On question, the images expected were said to be verbal.] A 22. Very vague memory image of newspaper statement; I don't know what kind it was. I think the memory image fuses with the statement, and that is the acceptance. A 10. Acceptation came in fairly definitely and with moderately pleasant feeling, since the conflict of ideas gave way to the fusion of them.—As to the nature of the fusion, we get a hint, not in the case of the 'new ideas and memory ideas,' but in that of the memory ideas themselves. A 9. The word Beauty was accompanied by a vague background of a host of ideas (verbal-motor in terms) fused together like the tones of the same chord played by different hands in different octaves on the piano. [This analogy would come readily to a highly musical observer.]

(5) A 90. Slight belief. . . This was slightly pleasantly tinged. A 22. Slight pleasantness, may be due to the fusion or assimilation. A 9. Accepted with a slight pleasant feeling. . . Unpleasant feeling accompanied the clash of different [verbal] images aroused by the different parts of the sentence. A 77. Not true absolutely, but may be true on the average. . . The whole state was slightly unpleasant.

Wherever disbelief was pleasant, the pleasantness was of a humorous nature, and found expression in a smile. A 60. At once the reaction set in, expressed by the phrase: That is nonsense, with smile of humor, and pleasantness.

(6) A 3. Agreement was established with a strong pleasant relief. A 13. Belief concludes with a slight pleasantness and a feeling of relief as if from muscular strain. I don't know what muscular strain there was.

Observer F

With this observer we made 187 successful tests. The analysis of belief-disbelief was simple and straightforward: the consciousness consisted (1) of verbal ideas, or (2) of kinæsthetic expression, or of both these factors together. Belief was characterised (3) by a peculiar organic sensation. Only occasionally was reference made (4) to affective processes.

(1) A 133. Acceptance came as: Yes. Also I found in myself a tendency to nod my head for acceptance, and to say again 'yes, that's right.' Acceptance is certainly, in this method of experiment, bound up with verbal ideas. E 7. I thought: Yes, that sounds O K. . . The belief was essentially the Yes. A 6. My acceptance was a verbal one, principally; I thought: That's all right. A 120. Disbelief. . . Tendency to say: It does [fluctuate]; it may be strong or weak. B 3. I thought: Yes—always. Belief was nothing more than the meaning [of the sentence] and the words thought. . . Belief came again; it was the meaning and a nod of the head. . . Belief came a third time, when meaning of whole was grasped, and was verbal: Yes, that's true.

(2) A 133, B 3, as above. Reports of nodding and shaking the head were very common.

(3) The organic complex was at first indescribable. A 6. Something unanalysable and faint; probably organic sensation; at least I can't think of it as anything else. [Later on it was described.] A 132. There has been in my case also a very faint, very obscure and uncertain organic sensation, coming from just below the ribs a little to the left of the centre. I have mentioned this before but have not described

it, because I was not certain of its nature. [The complex felt like a long narrow slip of pressure. It was not an invariable constituent of the belief-consciousness, but it was reported as constitutive where it appeared, and it appeared frequently with different methods. There is only one suggestion of anything similar in the case of disbelief. A 123. Failure to accept appears to me to be in this case mainly a 'feeling of unwillingness' or 'feeling of inability' to say the thing is true. But I am sure that the inhibition of speech is not all: something organic?]

(4) A 6. Belief. . . . Feeling was pleasant, and like the feeling of familiarity, if not identical with it. A 120. Feeling of dissatisfaction, both with the statement and with the author of it.—These affective reactions were very rare.

Observers Cl and P

With the observer *Cl* we made 48, and with *P* 24 tests. The numbers are too small for a final analysis; but we may note a few points of interest in the introspective reports.

The observers are of radically different types. *Cl* is pronouncedly visual, even more so than *C* (the observers, though of the same name, are not related). The understanding of the sentences consisted in the arousal of complex visual imagery, and *Cl* repeatedly said (1) that he could not distinguish understanding from believing; belief was given, at once, with the clear visual images. Disbelief came, not at once, but as a result (2) of the incongruity of the images, which was felt rather than perceived. Both belief and disbelief were intrinsically pleasant; if disbelief was unpleasant, there was a special reason for the change of affective process.

P was of the same type as *G*, and spoke always of belief and disbelief as a fitting-in with or contradiction of pre-existing ideas; the experiments ceased before she was able (3) to analyse this attitude. For *P*, too, belief and disbelief were both intrinsically pleasant.

(1) B 4. Understanding and belief were very closely connected. As soon as I had passed the words 'typhoid fever' I understood and believed. A 135. I am not sure just where the dividing interval between understanding and belief lay, or if there is any such interval. . . . I think that the instant I clearly understood the sentence, I also believed it. A 133. Understanding and belief were simultaneous.

(2) The following instances serve to illustrate the observer's wealth of visual imagery, as well as the fact that disbelief depends on their incongruity. B 2. I had a series of visual images, corresponding to cat, house, garden, a second cat, etc., and put them together piece by piece as in a picture until I had, as it were, a series of pictures,—first the cat alone on the background, then the bones alone, then the cat carrying the bones, etc. The whole scene, except the second cat, was familiar. The word 'benevolent' called up a picture of a clergyman acquaintance, and the introduction of this incongruous person aroused my sense of humor and I had a tendency to smile. . . . The idea of benevolence in that connection was so incongruous that I refused to accept it. A 125. I recalled from memory a formicarium, but this immediately disappeared, and I imagined the ants as on the table

before me: several of them, scattered over a space perhaps six inches square, were blue or yellow, and were warning off another ant, black, who ran about wildly, and tried to get inside the space. Then the ridiculousness and impossibility of such a scene struck me, and I decided that I did not believe it.

(3) *P* speaks, in general terms, of 'satisfaction at the fitting in of the words to the unformulated content of my mind,' of 'a feeling of disagreement and a tendency to argue in opposition,' of 'something there out of harmony with my own views,' etc. Here we have clean-cut instances of *Kundgabe* as opposed to psychological *Beschreibung*. Neither *Cl* nor *P* had had the general introspective practice of the previous observers, and the present experiments were, as we have said, too few to give positive results. Nevertheless, *P*, who is of the imageless type, is content with a form of words that conveys meaning but fails to describe, while *Cl* is saved from this substitution of logic for psychology by his strongly visual type. It is doubtful, however, if *Cl* is reporting a belief-consciousness that is on the same level with that of the previous observers; the identification of clear understanding with belief reminds us rather of Bain's primitive credulity.

It may be added, in illustration of type, that the observer *Cl* has schematic visual images which carry the meanings of belief and doubt. These images came out very clearly in the later experiments by the method of paired comparisons. C 14. In reading these two, I visualised a small stone sphere, like a marble, next to a larger, more irregular, but generally spherical body like a sponge, the latter being above the former. . . . In all these cases the marble represents the more concrete and the more easily understood sentence, the sponge the other.—In reply to questions about these images, *Cl* reported that belief was always represented, for him, by a circle or a ball, of small size, two or three feet away from the eyes, which is very sharp in outline and seems very heavy. Doubt or hesitant belief may be represented by a larger, softer, vague, indefinite and hazy ball. The schematic image for doubt is, however, less constant than that for belief. It may be replaced by other, more pictorial images. B 4. In cases of hesitation I often get a visual image of some familiar country road, one cross-road appearing more often than the others. This picture disappears when the sentence has aroused visual images of its meaning. B 16. I had the impression of being surrounded by a gray mist, which I could enter, but in which I was absolutely lost. C 13. I took the attitude that here was something definite which I had better accept, while my own definition was rather vague and I had not cleared things up. I visualised this as a person standing on a gangway above the water [the clear definition], while near by on the water floated a mass of wreckage or pieces of wood [*Cl*'s own previous ideas].

METHOD OF SINGLE EXPOSURES: MATHEMATICAL EXPRESSIONS, VISUAL

In this series of experiments we sought to analyse simple forms of the consciousness of certainty or uncertainty. Problems in arithmetic and algebra, of an easy kind, were type-written on slips of paper. Sometimes no solution was given. The observer was then instructed to read the problem, to solve it mentally, and to introspect the consciousness of certainty or uncertainty which accompanied his result. Sometimes the answer was printed on the same slip; it was covered, at the

beginning of the experiment, by a blank sheet. The observer was then instructed, either to read the problem, to look at the answer, and to verify its correctness by mental computation; or to read the problem, to solve it mentally, and after solution to compare his own answer with that given on the slip; in both cases he was to report upon the resulting consciousness of certainty or uncertainty. A full series of experiments was performed with the observer *C*; brief series were taken with *V*, *G* and *F*.

Observer C

With this observer we made 150 tests. (1) The problems were worked out in visual terms; as a rule, the visual images were clear and definite. (2) The reaction of certainty-uncertainty was more intensively affective than that of belief-disbelief in the preceding experiments; certainty was invariably pleasant, uncertainty unpleasant. (3) Kinæsthetic sensations were more strongly in evidence than in the former experiments, though they are still not a marked or uniform feature of consciousness. (4) The most important and characteristic feature of the certainty-uncertainty consciousness was a visual perception of agreement or disagreement. (5) This perception was clinched and expressed in verbal form.

(1) A 123. I added the numbers as they came with clear visual images of the series. . . The images were so clear that I could not have been much more sure if I had seen the scale before me and counted them out upon it. A 121. I used the visual method. . . The visual images were not clear, and the number that I was to add would sometimes disappear or get mixed with others. A 119. I read the problem and worked it mentally three times, using visual images. A 113. In the first part I had images of the numbers written under one another and proceeded as in ordinary subtraction. In the last part I had an image of the series as numbers on a line marking off distance, with specially large marks at each hundred and smaller ones at the tens. . . I did all the work by means of visual images.

(2) A 123. Affective mood pleasant. A 113. Slightly pleasant feeling; attitude of satisfaction. A 67. The whole process was agreeable. A 121. Uncomfortable feeling; dissatisfaction. . . Pleasurable feeling connected with the agreement. A 112. Very unpleasant. A 71. There was something unsatisfactory and displeasing about the result.

(3) A 121. Strain in eyes and forehead. . . Relaxation of strain [with later agreement]. A 71. Muscular strain about face. A 111. Sensations of strain in the eyes. A 119. Feeling of strain in eyes and forehead. Slight tendency to move the eyes as I followed the imaginary line from 100 to 28. Relaxation accompanied certainty. A 114. Organic sensations from breathing; strain in the forehead and eyes. (Uncertainty.) A 110. Feeling of certainty. . . I breathed more easily; the strain in the eyes is gone.

(4) A 123. I had a high degree of certainty that my answer was right the first time, due I think to the clearness of the visual images and the ease with which they fitted into each other. A 121. I had a

vague image of my numbers on the visual line overlapping or failing partly to meet. I was in doubt about the correctness of my answer. A 136. I was fairly certain of the answer the first time because each separate addition had been clear cut and had itself carried certainty. It is hard to say just what I mean by this. I think the visual image had most to do with it. When I am certain I am right, I can see very definitely to what point on my visual scale the series reaches; when I am not certain, it is less clear, and seems to overlap several points. A 137. Each step was clear and definite in consciousness and was accompanied by a feeling of certainty. This may be described as visual agreement. Each step of the process seemed to fit into the scale and to leave no doubt.

(5) A 113. Verbal images: Second was right. A 112. Verbal images: First probably right. A 110. Verbal image: Mistake? A 114. Verbal images: This is right. A 115. Verbal images: No use to do it again; this is right.—The phrases: First right, Second right, Where's the mistake? etc., were very common.

Loss of interest. It is worth noting that, as soon as the consciousness of certainty had formed, the observer fell into a specific attitude, which she designated 'loss of interest.' A 136. I lost interest in the problem and had no desire to do it again. A 115. Loss of interest in the problem and difficulty in keeping the attention on it any longer. A 118. Loss of interest in the problem. A 117. The problem lost interest. My attitude might be called a feeling that it was of no use to try it again as the answer would certainly be the same.—We have had no opportunity of subjecting this attitude to analysis. Its main feature appears to be a motor relaxation and restlessness, corresponding to lapse of voluntary attention.

Observer V

With this observer we made 50 tests. We found, as before, kinæsthetic and other organic sensations of an empathic kind, and we found (1) that complexes of organic sensations were the essential factor of the certainty-uncertainty consciousness. (2) Certainty and uncertainty are characterised by a fairly intensive affective process, pleasant for certainty and unpleasant for uncertainty. It is, again, interesting to note that verbal images (aside from the names of the symbols) were not once reported. (3) The work is usually done in visual terms.

(1) A 116. A host of organic sensations; pressure on the chest especially. . . . The feeling of certainty was distinct: I felt strong and sat up straight. A 120. Many organic sensations in chest, arms and back. Sat up, breathed deep. Had a sense of power. (Certainty.) A 136. Felt certain. . . . Got sense of power and superiority. A 144. Feeling of certainty. Organic sensations and usual feeling of power. A 140. Feeling of uncertainty: heavy pressure in chest and arms, felt tired, breathing difficult, frowned. A 142. Feeling that I could not trust myself: sensations of pressure in chest and back, felt tired. A 118. Feeling of helplessness; no certainty at all; creepy feeling in back, and pressure in chest and stomach; felt tired. A 143. A feeling of uncertainty. Mostly organic sensations of pressure in different parts of the body; felt chilly. A 117. At first uncertainty: shivery, prickly sensations in arms. Then certainty: sat up straight. Feeling of safety, which I can't analyse, but I think breathing had most to do with it.

(2) A 119. Pleasant. Sat up, breathed deep, felt satisfied. A 136. Felt certain. . . Restful and pleasant. A 141. Very pleasant and restful. A 140. (Second stage.) Decided feeling of certainty and elation. . . Had feeling of power and satisfaction. A 139. No certainty. . . Very unpleasant. A 142. (See above.) Rather unpleasant.

(3) A 139. Visualised but kept forgetting. A 142. Visualised, but the figures moved; unsteady. A 120. Visualised and did it that way; very easy. A 140. Visualised and made it more easy by moving my pencil in the air as I worked.

Observers F and G

With these observers we made only 20 tests. The following introspections supplement and confirm those already cited under Belief.

F. A 66. Certainty this time seems to be a sensation in back of neck which comes when I nod my head; also an organic sensation located near the place where the organic sensation of belief is, and much like it. A 115. Nodded my head. . . Organic sensation in lower chest, which seemed to be a feeling of 'I thought so' or 'Just as I thought.'—Verbal ideas, of the kind already quoted, were very common. The experience was more definitely affective than in the previous experiments; certainty was pleasant, uncertainty unpleasant.

G. A 141. Distinct feeling of relief with long expiration (must have been holding my breath while computing); said aloud: Yes. Bodily position was changed in relief; straightened out. A 117. Feeling of certainty very strong, expressed itself immediately in the spoken sentence: This must be right. Pronounced pleasantness, and muscular tension in face, as if inhibiting a faint smile. A 142. I was sure (I don't know how to analyse it) that my calculation was right. . . The whole task was verbal, auditory-motor.—Internal speech (or spoken words) and the subsequent emotion of relief are again characteristic of this observer.

METHOD OF SINGLE EXPOSURES: AUDITORY

At the conclusion of the visual series, we made experiments in which sentences or mathematical expressions were read aloud to the observers by the experimenter. The observers were *C*, *V*, *F* and *G*; about 60 tests were made with each one. The introspections of *C*, *F* and *G* show no new features, and we therefore do not quote from them. We have, however, certain reports from *V* which are worth quotation, as they seem to throw light upon the analytical psychology of suggestion (religious conversion, etc.).

B 2. A flash of belief came at this moment. Seems to take hold of me and envelop me. I felt light and airy, cool, rested and relaxed. The sense of belief is not an intellectual thing. The reasoning comes first, and after the actual reasoning process is concluded the rush of belief comes with almost physical force. I feel that I believe, and the thought or idea of what I believe is not present at all. The belief in its momentary flash is not referable to any object. B 1. Then came a feeling of belief. It seemed to come upon me from

outside and envelop me like a gust of wind or a flash of light. I sat straight, felt refreshed and relieved and satisfied. B 4. I came through this [reasoning process] and stopped and believed. As soon as I believed, I forgot everything. Sat up straight, breathed deeply, felt strong. Pleasant. B 7. Then I had a decided disbelief, very clear and definite. It seemed to come from the outside. I had kinæsthetic sensations from straightening up, and organic sensations from long breathing, and I had a feeling of superiority. B 11. Disbelief came on me as a strong wind from the outside.

Several reports of Doubt were obtained in this series. B 8. Then understood and felt doubt, with sensations in chest, frowning, heavy feeling in arms. B 9. Doubt, as organic sensations in chest and arms, turning head and squinting of eyes; breathed with difficulty. Unpleasant.

METHOD OF PAIRED COMPARISONS: SENTENCES, VISUAL

Our observers were now trained to the fulfillment of the *Aufgabe* or problem of the experiments, and instances of belief were becoming far more common than they were at the beginning. We thought, however, that the method of single exposures could yield no further results of importance, and we feared that the introspections might take on a stereotyped form. Hence we had recourse to a method of paired comparisons. Sentences, so chosen as to evoke belief and disbelief, were typewritten upon slips of paper. The observer was instructed to read them, to note the appearance of belief or disbelief or both, and then to report introspectively upon the belief and disbelief consciousnesses. The regular observers were *V*, *C* and *F*; a few observations were also made by *G*.

Observer V

With this observer we made 246 successful tests. We need not again illustrate *V*'s tendency to empathic kinæsthesia. We note that belief and disbelief are characterised (1) by a fairly intensive affective process; if the belief and disbelief are strong, the affective accompaniment is almost always pleasant for both; if they are weak or but moderately strong, both consciousnesses show a ratio of pleasantness to unpleasantness of approximately 8 : 5. There is no trace of any regular distribution of the affective processes, pleasantness to belief and unpleasantness to disbelief. Doubt, however, is always unpleasant. (2) The most important factor in the belief-disbelief consciousness is an organic (mainly kinæsthetic) complex, the nature of which is shown in the following quotations.

(3) *V* again showed the threefold type of reaction: belief-disbelief, deliberation and verification, belief-disbelief once more. When decision had been taken, (4) there was muscular relaxation and the emotion of relief.

(1) C 13a. Clear, strong and pleasant belief. C 15b. Belief grew till it was maximally vivid, strong and pleasant. C 17b. Disbelief strong and clear. Interesting; . . . felt amused and smiled. C 14b. Belief very pleasant. C 20a. Belief was strong and pleasant. C 20b. Disbelief was strong. . . Feeling pleasant. C 59b. Got an amused but decided disbelief. . . Pleasant; smiled. F 4a. Got at once clear, strong disbelief. . . Felt disagreeable, cross and impatient. F 10b. (Disbelief.) This was pleasant, and disbelief differed from the belief of the other sentence only in its name.

(2) The 'feeling of power' was frequently reported in connection with belief and disbelief. C 41a. Strong tendency to disbelief. . . Felt strong. C 48b. Strong and decided disbelief. Felt superior and rather cross. Felt like pounding the table. F 2b. Believed it. Organic sensations of power, with deep breathing. F 11b. (Disbelief.) I had the organic sensations of power that meant I was sure my decision was right. C 22a. Strong feeling of power. Intense belief. C 9b. Disbelief. . . A sort of amused, lenient but decided sense of power. C 1b. Decided disbelief. Felt supercilious and haughty. F 25b. The feeling of disbelief was quite sure, and I had the feeling of power. [On question from the experimenter, the observer declared that she could find no psychological difference between belief and disbelief.]

In most cases, the 'feeling of power' was connected with the perception of straightening up in the chair. F 22a. Feeling of power and strength; sat up straight; deep breathing. F 14a. Frowned; held my breath. . . Felt sense of power; sat up straight. (Disbelief.) F 14b. Clear, intense belief. Sat up, smiled, felt strong. F 10a. This certainty of being right is carried by organic sensations of power. I sat up, smiled, breathed deep, puffed my chest out. C 49a. Disbelief. . . Took breath, sat up, and felt sense of power. C 29a. Felt superior, sat up straight, smiled. Disbelief clear. C 13a. Clear, strong . . . belief. . . Sat up, breathed deep, and had sense of power; satisfaction. C 20b. Disbelief. . . Felt haughty, and had sense of strength. Sat up straight.

Sometimes, however, organic sensations are reported without reference to this 'feeling.' F 25a. (Belief.) Organic sensations and motor image of nodding my head. Strong breathing and sensations from diaphragm. Kinæsthetic image of sitting up. F 19a. Intense belief. . . Organic sensations and kinæsthetic images of sitting up straight. F 13a. Belief . . . is clear and intense (deep breathing, sitting up straight, etc.). C 28a. Decided sense of belief . . . Felt satisfied, sighed, leaned back in chair. D 10a. Felt sure and satisfied. Sensations from sitting up. F 7b. Belief at once . . . I nodded my head, sat up straight, breathed easily, and felt comfortable.

Change of breathing is especially frequent with strong belief or disbelief. F 14b. Clear intense belief. . . Breathed easily. F 5b. Belief is clear . . . Long easy breathing. F 2b. Believed it . . . Deep breathing.

In the following instance of strong belief the 'feeling of power' is identified with kinæsthetic attitude. F 27b. Kinæsthetic image of nodding the head very hard. I had feeling of activity. Feeling of activity or feeling of power means sitting up, deep breathing, and all the other muscular and organic sensations that I have mentioned.

Nodding and shaking the head are fairly often reported. F 27a. I got belief without getting meaning clear. It was simply a tendency to nod the head. F 5a. Feeling of acquiescence; nodded my head a little in faint acceptance. C 58a. Shook my head emphatically,

meaning: No. F 3b. Disbelief consisted in rigid muscular state, shaking the head, and sensation of drawing away from something. F 14a. Felt confused: frowned, held breath, shook head.

The organic sensation of tingling is peculiar to this observer. F 14b. Intense belief. Had tingling sensations all over me. F 20a. Especially tingling sensations in the spine, which I am likely to have with intense belief.

Strong disbelief is also evidenced by bodily stiffening and shrinking: cf. F 3b above. F 4a. Rigid muscles, with nervous movements of hands and feet; rapid breathing; drew back. F 6b. Shaking head in dissent; shrugging back with shoulders, twisting mouth, breathing out rapidly. F 1a. Sensations from breathing fast, with muscular strain especially in face; motor image of shaking head; stiffened and drew back. F 14a. Shook head. General muscular contraction; kinæsthetic image of stamping foot.

The 'feeling of power,' in its intense forms, is characterised as a feeling of aggressiveness. C 29b. Felt aggressive and rather angry . . . Felt actively opposed and displeased. Unpleasant, breathed fast, felt hot, frowned. C 8a. Felt a sense of power and aggression. Felt angry and grew hot. Disbelief very strong and decided.

Verbal ideas were infrequent.

(3) F 6a. Read it twice. . . Smiled, and got feeling of disbelief. [Stage of wonder and thought.] Felt that I was right, and the disbelief-sensations returned. F 12b. Intense belief at once. [Then stage of forced or voluntary deliberation.] Feel that I am right. F 14b. Intense belief. [Stage of consideration.] Belief was very intense and clear. F 19a. I got belief before I stopped to get meaning. [Consideration of meaning.] Satisfied I was right.

(4) C 14b. Belief . . . Calm and comfortable. I leaned back and felt rested. C 41a. (Disbelief.) Relief; felt rested and strong. C 45a. Belief . . . Felt amused and contented with myself. Felt rested. F 1b. (Belief.) Organic state peaceful. General feeling of muscular relaxation. F 5b. (Belief.) Restful relaxation of muscles. F 19a. Satisfied I was right (this consisted of muscular relaxation, and relief).

I used various adjectives to characterise her beliefs and disbeliefs. All of them may, according to her own subsequent report, be reduced to the two pairs strong-weak and clear-vague. 'Solid' and 'decided' were employed as synonymous with strong; 'distinct' and 'definite' as synonymous with clear. The intensity of belief varied with the intensity and massiveness (spatial distribution) of the kinæsthetic and other organic sensations with which the introspective records have made us familiar. The clearness or definiteness of belief depended, in logical terms, upon the relation of the statement to pre-existing knowledge. A belief was clear if it stood in associative connection with particular memories, which justified it. Usually, this relation, so far as it is conscious, is for the observer a kinæsthetic attitude; in the present experiments, a visual image (less frequently, a form of words) tended to replace the attitude. Sometimes, however, the clearness of the belief was kinæsthetic; so that we have the very curious fact of two kinæsthetic attitudes—those of

belief itself, and of the clearness or definiteness of belief—somehow combined, or superimposed the one upon the other. We regret that we have been unable, from lack of time, to trace this complexity further.

F 14b. (Belief was intense and also clear.) I knew why I believed, and this was what made it clear. Got visual image of philosophy class; this seemed to mean 'If religion is properly interpreted, it need not conflict with science at all.' F 7a. The clear element is what I call the logical element; I know why I disbelieve. The intensity of disbelief depends on the organic sensations, and on whether I feel I am justified or not [this 'feeling of justification' has been analysed in previous reports].—On question from the experimenter: The clear element came from memory of lessons in biology. I think I visualised the text-book. F 10ab. On reading first statement I had a sense of familiarity. Got successive visual images of the observatory at Vassar where I saw sun-spots, and of several people I knew. . . Belief was clear because I knew why I believed it; had learned the fact in elementary astronomy. . . The clearness of belief comes more from a logical, the intensity from a psychological process. . . On reading second statement . . I got immediately clear, strong disbelief. I had the sense of familiarity, and had visual image of a telescope, and of the moon as seen through it. . I knew why I did not believe it; I had been shown good proof of the impossibility of life on the moon. I felt (organically as before) that I was right.—On question from the experimenter: The clearness came with, or was attached to, the visual images. My sense of familiarity is usually kinæsthetic and organic, apart from the affection; I think these sensations were there too, but I didn't pay attention to them. F 26a. Then I got verbal association of some experiment done in a woman's college. . . Then I was inclined to disbelieve. Disbelief was not intense, but quite clear . . . I understood why I disbelieved. . . Weak and intense belief differ only as regards intensity of kinæsthetic and organic sensations; they differ in degree. Intense belief can be more easily localised; strength of belief is muscular strength.

In the cases quoted, we find visual or verbal images that carry the clearness. The reason that the observer speaks of clearness of a 'logical' matter is that oftentimes a belief 'feels' clear, touches off the verbal report 'clear,' without there being anything in consciousness—or at least, anything discoverable by the present method—to serve as specific vehicle of the clearness. A clear belief is a belief which could be justified later, by explicit argument, if the observer were challenged to justify it. We are reminded of Störing's 'state of assurance,' which may apparently exist only as a physiological disposition.¹

Observer C

With this observer we made 225 successful tests. The most important feature of the belief-disbelief consciousness was, again, (1) a perception of visual agreement or disagreement, which (2) was clinched and expressed in verbal form. (3) Kinæsthetic sensations and organic sensations were more in evidence than in the first series of experiments, though they were still not a marked or uniform feature of consciousness.

¹ Cf. Titchener: *Thought-processes*, 1909, 289 f.

(4) Affective processes were neither frequent nor intensive; belief was pleasant, disbelief pleasant or unpleasant.

(1) F 14b. (Belief with reservations.) The belief and disbelief were carried partly by Yes and No. But the main thing was something visual; hard to describe. I had two long lines, moving parallel to each other: each one had branches, hitting those of the other. That was conflict; the main things did not conflict. C 42a. Belief. This is hard to describe. There were visual images, more or less fragmentary or schematic, representing peculiarities of poets, and these were somehow seen to agree with the statement. F 27ab. Belief in both cases. . . Visual image of adult, and then of child; reference to my own experience. That was a line representing the time from the present to the past; there was nothing more that I can tell. . . Memory of child was very plain, but scattered and disarranged; a visual pattern made up of bits, which were very clear, and which I could see very plainly; clear cut, and separate from one another. Memory of adult was like a spider's web, clear, all united. . . All this fitted in, in some way, with the reference to past experience; I believed; the two things fused together; but I can't say whether at this stage there was any representation of past experience.

Sometimes a visual schema for 'past experience' is reported. D 4a. I had visual images representing will, past experience, etc. . . . When I say I believed, I cannot say that there was anything in consciousness except the mere visual image representing the meaning of the sentence, which was seen to agree with the visual image of previous opinion. This latter was present at first as a dark, vague object in the visual field; after I began reporting, it translated itself into words. C 48a. There was something in consciousness which represented the interpretation of animal action in human terms. It is hard to say what it was. I am sure that it was in part at least visual, but I cannot describe the image. It seemed to agree with the statement; it was a visual perception of this agreement. C 7a. It is very hard to say in what this belief consists. I usually have a schematic visual representation, sometimes very vague, of previous opinions on the subject, and seem to see an agreement between that and the present sentence. The assent is partly verbal sometimes, often expressed by the one word Yes; but I am sure that this is not always present. D 9b. I believed the statement; but it is hard to say what this consisted of. I think it was mostly visual. My schema of feelings of familiarity was seen to coincide with that of the feelings named. C 2b. On reading I had a schematic visual image which represented evolution and progress to me. It was very vague and I can't describe it. I assented; the assent or belief seemed to come as agreement of this schema with that of previously accepted opinions. D 7a. Visual image of two parallel lines, at first in some large, vaguely seen plane in space, then on a page of the geometry I used to study. Visual image of the outside of the book, and verbal image: Wentworth. Verbal image: Euclid. In the image of the lines I seemed to see that they were just as far apart from one another in one place as in another. The lines as I saw them did not meet. My belief consisted of some perceived agreement between the statement on the paper and that of my geometry. I cannot remember anything in consciousness, except the visual imagery, that carried the belief. C 43a. Visual image of a mob in the street. At first identified as a scene in the Tale of Two Cities; then it changed to a scene in Felix Holt. When I say it was a scene in these books, I don't mean that I said the names of the books till I came to report. The scenes in either book seem like

independent things to my imagination, and I see them in their place and know what they are without any verbal image. Then visual image of Calkins' Psychology and memory, I think wholly visual, of a passage in it. Belief. This seems again to have been a perceived agreement between the sentence and my images.

The two last introspections have been given with some fullness, in order to remind the reader of the typically visual character of *C*'s thinking. We proceed with descriptions of belief. *F 4b*. Visual image of a man hypnotising a subject by waving his hands before his face. As this seemed to fit into the schema for 'absence of any stimulation,' I believed the statement. I cannot describe this belief further than as a coinciding of the visual schemas. *F 1ab*. It is hard to define the difference between the certainty itself and the contents because they were so closely connected. The content came as a visual schema, and the certainty seemed to consist in the fact that it fitted a previous schema in my mind. I did not see the two separate at first and then coinciding; but there were no ragged edges; they coincided. *C 14a*. The belief was only slightly if at all verbal. It consisted of strong organic sensations and of what I can only describe as a perception that the schematic visual image produced by the reading fitted those already in the mind. I know that this description is not accurate. I do not mean that I first had the schema of my own opinions, and that the new schema then fitted into it, or that they were both there and moved into each other like the double images in stereoscopic vision. There was the one representation; but there was nothing outstanding that did not come under the statement. *F 12ab*. The belief here was expressed by a partial agreement with the schema of my ideas, and by a vague image of a book from which the statements were taken. This was especially clear in the second sentence, where the agreement was followed by the image of a book which I read last week and in which the ether is discussed. *F 3ab*. On reading first statement there were in consciousness only the verbal images of the words and a vague visual schema. There were the same in the second and, besides, a reference to Marbe's work on judgment. I think that this reference was visual, that is a visual image of the book and of the name Marbe as printed. There was also a vague visual schema of Marbe's conclusions, and outside this, below a straight line which separated the two, an image representing the Aufgabe. If the feeling of certainty was something distinct from the facts I have given I do not know how to describe it; I find nothing else. In the first there were no ragged edges in the visual schema; it seemed to fit my previous ideas. In the second, the Aufgabe below the line represented in some way an exception to the statement. *D 1a*. Belief came immediately, but it is hard to say in what form. It seemed to be what I call perceptual. I saw myself in the dreaming state and saw that that and my present state did not agree, just as I might see that the chair and the table are not the same thing without any explicit process by which I arrived at this conclusion.

C 36b. Visual representation of the two sentences at the opposite ends of a line; this meant: They are contradictory. *C 31b*. Visual image of some nerves, with the idea of a current passing along them; I think this was mainly visual too, in the form of undulations. Verbal ideas: Soul, and Electricity. Disbelief. This was mostly visual. It was a perception of the incongruity of the two; the visual images did not coincide. *D 8b*. (Disbelief.) I think it was mostly visual. My image of the elasticity of the brain-cells did not suffice for intelligence; did not cover it. There was a dark hazy halo round the outside which represented the rest of intelligence. *C 41a*. Verbal idea:

Perhaps not. This was accompanied by a vague visual image of an ill-defined dark mass which seemed to represent other possibilities not here considered. The meaning was that there were other things to be said; but this was not expressed in words. F 2a. Verbal idea: Dangerous. Visual images of something cut, but still mingled up again. In one way I believed, but in another way I did not believe. F 19b. Visual images, not only of the body itself, but of bodily processes. I had the contents of the sentence visually in mind, and I had visual images representing substance and power;—dark spots, very vague. The pictures seemed opposing; I did not believe; unpleasant affective tone. Then I had verbal ideas; just two or three words, I can't remember. Then the opposition of the images disappeared; they fitted together; and I believed the statement. F 8b. Mental contents were visual images. Belief consisted in their nearness together; there was also agreement with past schema. D 2ab. As between these two sentences, the first seemed much more definite and nearer. The image in the second was vague and very far away and inaccessible. The distance between it and myself seemed to offer an insuperable barrier, like the distance to the stars. With the first, though it was definite and near, there was a sort of background of doubt, a cloudy something that seemed to lead out in all directions and indicated that influences in human character are hard to measure. In reporting, this came to me verbally, but it was a visual image of a cloudy substance that seemed to radiate in wide, blunt strands in every direction from the central image.

(2) F 10b. Distinct visual images of the moon, and of living beings walking round on it. Verbal idea: Don't know. Then: If there are such beings, the telescope might reveal them. I think the last was not completely expressed in words, as here; it was partly visual; I seemed to see a telescope directed upon them. In these cases the visual images constitute the mental contents coming from the meaning of the statements, and the verbal ideas and visual agreement or non-agreement with the other schema constitute the feeling of certainty. C 24a. Visual image of a negro. . . Then visual image of a professor of sociology. . . His statement was clearly present in the form of visual images. . . Neither belief nor disbelief on first reading; on second reading, verbal ideas: They are the same, meaning inborn difference and heredity. Noticed word: Social. No decision; feeling of confusion; sensations from breathing. Later disbelief, which came in the form of words; something like: All heredity is not social; though more scrappy.

Brief verbal images: Yes, No, Probably, Perhaps, Indefinite, Clear, Of course, On the whole, etc., were exceedingly frequent.

(3) F 1b. Belief. . . Organic sensation from region of the diaphragm; I think, with the belief, though it may have come later. C 14a. Strong organic sensations from region of diaphragm and higher up in the lungs in connection with breathing. (Belief.) F 19a. Belief was just visual fitting; there was one image, and there was fitting; prevailing pleasure, and organic sensation in diaphragm. These all seemed to be mixed up together.

Reports of this sort were rare. The sensations from nodding were occasionally reported; never those of shaking the head. F 21a. Slight tendency to nod the head, and repeated nodding as I went on reading. F 6a. Very slight kinæsthetic image of nodding my head. This is the only fact that I can give as belonging to the assent.

(4) C 8a. Very strong disbelief. . . Slight feeling of amusement and tendency to smile, with accompanying sensations from face. C 9b. Strong feeling of disbelief. . . Feeling of amusement and tendency to

smile. F 19b. (Disbelief.) Unpleasant affective tone. F 15b. Black background with a mere chaos of figures and unpleasant affective tone. I don't know if the word No was present. . . I think the disbelief was carried partly at least, in some of these cases, by verbal images and affective process. C 15a. (Belief.) Attitude of satisfaction. This was a pleasant feeling, with a tendency to proceed at once to introspection and not delay to consider the question. C 16b. Assent. . . Present as a feeling of satisfaction and pleasure, accompanied by a visual schema. D 1b. The question was settled immediately. Belief; feeling of satisfaction; inhibition of further effort.

C characterised her beliefs as strong, weak, faint, definite, indefinite, sharp, clear, focalised. She made no such distinction between intensity and clearness as was drawn by V; possibly the difference in mental type may account for this difference.—The two first of the following reports were obtained by question from the experimenter; the third was volunteered by the observer.

C 4ab. The degree of belief was about the same in these two cases. There was some doubt in both; that is, there was a tendency to think of exceptions, which were partly visual, partly verbal in consciousness.—I can scarcely say that the nature of the beliefs was different psychologically. I thought verbally that *b* could be got from actual observation, while *a* must be more theoretical and a matter of individual opinion. But this, as a difference between the two beliefs, is a matter of logic rather than of psychology. It was present in consciousness, so far as it was present, in words. The conditions of belief in both cases included vague visual images. C 5ab. Belief slightly greater in *a* than in *b*. Here also the grounds of belief differed logically, and were expressed verbally (so far as present in consciousness). Verbal idea, like: Insufficient negative proof, in consciousness with *b*. C 15b. There was a difference between this belief and that of *a*. The latter was greater, but there seems to have been some qualitative difference which it is hard to describe, in psychological terms. That in *a* was more sharply defined, and decisive; that in *b* was broader and more vague. I believe that these terms apply to the background of organic sensations, but I am not sure.

Observer F

With this observer we made 184 tests. The analysis of belief-disbelief gave (1) verbal ideas, or sensations of kinæsthetic expression, or both of these factors together. Belief was characterised (2) by a peculiar organic sensation. (3) Affective processes are more frequent and stronger than in the first series of experiments. They are very variable, within the course of a single observation; on the whole, however, belief tends to be pleasant and disbelief unpleasant. They are often connected with a change of breathing.

(1) We begin with the verbal images. C 44b. Belief came at the first reading as No. That is, I believed the statement, but said No as negating a third mental element. F 2a. At the end of the first sentence I said No; I could feel the tone, so to speak, not like a mere kinæsthetic image, but like the actual sensation. Just exactly as if I

had actually said No. Sometimes No expresses belief and Yes disbelief. F 14a. At the end I said: No, these count (verbal-auditory image). The words: These count, stood for all that I had learned in ethics and philosophy. F 14b. I said Yes, with a sort of muscular set all over my trunk, especially along my sides. F 24b. As soon as I had finished reading I said: I don't know—I suppose so. Between the two there was a general emotional mood with a bodily set, meaning something like But. With the second term there was kinæsthetic image of a shrug of the shoulders; indifferent. The whole mood was a sort of acceptance. C 29b. Verbal No; complex of muscular and organic sensations that I should now interpret as: That's all nonsense. F 5b. Read, and said: Rheumatic babies (auditory-motor). There was nothing behind; that was the belief. I had had strains, particularly in shoulders and upper part of body in *a*, but I was relaxed when I came to *b*.—There were many cases in which a word, Yes or No, was the whole of the belief or disbelief consciousness.

F 3a. I said Yes to myself and nodded my head. C 31a. Belief came at once in a Yes, auditory-motor, with a muscular sensation in middle of back of neck. F 23a. Belief was kinæsthetic image or sensation; I don't know which. It was at centre of back of neck, very much like the sensation of nodding. C 1a. Tendency to nod my head. Had sensations (muscular) at back of neck. F 4b. Said No to myself, and actually shook my head.—Reports of this kind were very common.

(2) C 1b. Organic sensation when meaning was clear and belief came. . . Localised in the abdomen, a long narrow transverse and rather weak sensation. C 2b. Organic sensations, not localisable exactly. F 3a. Got usual organic sensations from abdomen.—These sensations never appeared with disbelief.

(3) F 7a. (Disbelief.) Rather unpleasant. . . While it was unpleasant, I had disturbance of breathing. F 10a. The verbal images, the sensations from change of breathing, and the unpleasantness seemed to make up a single complex. (Disbelief.) F 1b. Said: Of course; rather pleasant, with slight relaxation of breathing. F 2b. Said No; unpleasantness and breathing strain. Said Nonsense; had strains in arms and shoulders, and very definite set or bodily posture. Unpleasant. F 5b. Disbelief, quite sure, rather unpleasant. C 23b. Affective processes changed so often that I can hardly describe them, but disbelief was rather unpleasant, while belief was pleasant. F 13b. Said No; rather unpleasant. At the same time, organic sensations from breathing, such as I might have if I heard: You're a liar, only weaker. Weak muscular strain in chest.

Observer G

Only some 20 observations were made by this observer. We append a few characteristic introspections.

G 1ab. Consciousness mostly verbal. In reading first sentence, tendency, verbal and kinæsthetic, when I came to the word *goes*, to say: Does not go. . . After finishing second sentence, two tendencies, kinæsthetically to nod head, and verbal-motor as if to say: That's all right. I can't analyse the verbal tendency more. It was simply an innervation of the tongue, and sometimes larynx, to pronounce the first sound of each word. G 5b. Nodded to it and said Yes to it. G 7a. Verbal ideas: No, Bismarck is greater, Gladstone is greater; don't believe it. Only partly articulated; I wanted to whisper it. G 7b. Tendency to smile and to say: That is absurd. Tendency to smile consisted of strains in the face, especially around the eyes; with a flash there came a host of verbal ideas, in shorthand, about the dis-

turbance in the Orient; too complicated and vague to be analysed. G 12a. All I remember is a vague verbal tendency, meaning: At least business men believe it. This was very much abbreviated. G 5a. Host of verbal associations, abbreviated and condensed; one of these was a change of the sentence into: Drinking makes some of us happy. That too was abbreviated. Then there was a very vague, kinæsthetic and motor attitude, representing a woman of the total-abstainer kind, with disgust for drunkard, turning away her face and head and wrinkling her forehead: these things seemed to occur in my own case (feeling of disgust, tendency to turn head and wrinkle forehead). There were words in this too. [This empathic kinæsthesia is rarely reported by G.] G 12b. Very vague sort of visual image of grocery store on College Avenue, with vague square show window; vague image of clerk with long apron. Tendency to smile, with kinæsthetic and motor attitude as if I was buying in that store.

METHOD OF PAIRED COMPARISONS: MATHEMATICAL EXPRESSIONS, VISUAL

We introduced two modifications into the procedure of the introspective method. The one of these was that we asked the observers, at the conclusion of the principal series of experiments, if they could suggest any further work that might throw additional light upon the nature of the belief-disbelief consciousness. The observer C, in response to this question, suggested that a short series be taken, by the method of paired comparisons, with mathematical expressions instead of sentences.

Observer C

The observer gave 50 observations. (1) The certainty-uncertainty consciousness was strongly affective; certainty was pleasant, uncertainty unpleasant. (2) Kinæsthetic sensations are present in many cases, though they are not prominent features of the records. (3) The most important thing is a visual perception of agreement or disagreement, which is (4) clinched and expressed by words.

(1) G 25a. Felt quite certain. Satisfaction. G 26a. I was quite certain; pleasantness was contained in it. G 42b. I had vague schematic image of possibility of making a mistake, and it was very unpleasant. G 43b. Frowned; it was very unpleasant . . . I got the feeling of uncertainty; unpleasantness. G 41b. Very easy and pleasant.

(2) G 32a. Certainty . . . Relaxation of muscles; sat back. G 34a. Dissatisfaction and unpleasantness; restlessness, and general muscular strain. G 41a. Tension of muscles. (Uncertainty.) G 41b. Feeling of relaxation. G 43b. Strain in hands and upper part of body . . . I got the result; nodded.

(3) G 40b. Since the number was so large, and contained so many terms, I always forgot the preceding images when I came to the next stage . . . Lack of clearness; this was simply confused images of a lot of numbers. Also very vague schematic images of those places where I might have made a mistake. G 25a. Visual images were very plain; clear cut. I could see it was right. Everything was very clear visually; the numbers were at their right places on the scale. G 26a.

Certainty was caused by clear visual image; there was no overlapping. Saw number-scale for numbers higher than 100. G 42b. Tendency to try it over again, that is, to start in again. When I had another answer, I had both answers in clear visual images; but they looked entirely different. I can only say that the one looked as if it fitted. [The observer remarked, G 30a: I don't know how to describe the tendency to repeat, to start over again. It is anyhow partly visual. Very vague visual image of possibility of getting another answer. It is too vague to describe. There seems to be kinæsthesia behind it. G 43b. Strain in hands and upper part of body, which composed the tendency to do it over again. Sometimes this set was more prominent, sometimes the images were prominent; vague visual schema of chance of mistakes; a big confused mass with everything mixed up.]

(4) G 42b. Here images were so fleeting that I could not use them; I did not feel sure at all. Hence I worked both visually and verbally. G 43b. I said the numbers to myself. G 40b. Performed partly visually and partly verbally. G 32a. Clear images, visual and verbal. G 24a. Idea that I might have forgotten, might have been mistaken (partly verbal; also visual image of something that had a blank space).

EXPERIMENTS WITH TONES

At the request of the observer *F*, we made a series of 50 experiments as follows. The experimenter took two tuning-forks, of 256 and 260 vs. respectively, and after a signal struck in succession either the two forks (A-B or B-A) or the same fork twice over (A-A or B-B). Having struck the forks, he pronounced the word Same or Different, irrespectively of the actual relations of the tones. The observer was to react to this judgment expressed by the experimenter, by way of acceptance or rejection, and was to give an introspective account of his consciousness.

The introspections were complicated by the kinæsthetic attitude of expectation. As regards agreement and disagreement, we find (1) that, almost without exception, *F* has verbal ideas: Yes; No; Yes, same; No, same; Yes, different; No, different, etc. (2) Further, in the majority of the reports there is nodding or shaking of the head, actual or imaginal. (3) The affective accompaniment is marked: agreement is always pleasant, disagreement always unpleasant.

As these results simply confirm the previous analyses, we do not print extracts from the introspections.

FINAL INTERROGATION OF OBSERVERS

The second modification of the method that we introduced is that, at the conclusion of the whole investigation, the experimenter reduced the introspections to a single formula, which was laid before the observer for approval or disapproval. We regard this procedure—a sort of procedure by confrontation—as important in work of the kind here reported. The experimenter amasses a great bulk of introspective material, which he has to sort, evaluate and arrange, and from which he

has to draw psychological conclusions. By rights, perhaps, the whole of this material should be printed as an appendix to his paper; but no Journal has space for it. There is always a risk, then, that the experimenter put upon the records an interpretation which is not shared by the observers themselves: witness the classical instance of Bühler and Dürr. If, however, he read to the observer, when the work is completed, his digest of the reports dictated, then the observer has an opportunity to say his say independently; he can declare his agreement or disagreement with the conclusions. His approval must greatly fortify the whole argument of the paper; his disagreement should lead to further study of the consciousness in question.

Our observers did not know beforehand that this method would be followed; but all of them expressed their approbation of it when it was actually employed. We proceed to the details.

Observer V

The experimenter read to the observer the following summary statement. "For the observer *V*, belief-disbelief is characterised by fairly strong affective processes, which tend towards pleasantness. The affective process accompanies a characteristic kinæsthetic-organic complex, which forms the core of the belief-disbelief consciousness; if a single name is to be given to this complex, we may term it a 'feeling of power' or 'of aggressiveness.' Verbal ideas are rare in the consciousness; the contents of the belief are usually visualised. In the case of certainty-uncertainty, we find the same type of consciousness, except that certainty is pleasant and uncertainty unpleasant." The observer unhesitatingly accepted this analysis.

The experimenter then asked whether *V* had ever come across anything in the way of an imageless content, either in belief-disbelief, or in any other consciousness accidentally aroused during the course of the experiment. The answer was negative.

The observer was then asked how she distinguished, psychologically, belief from disbelief. She replied: "I don't know, except that my feeling of power means sometimes arguing for and sometimes arguing against. Perhaps a variation of the method would bring out some difference in the feeling itself, but I don't think so. I was working under the alternative *Aufgabe*, belief *or* disbelief; the consciousnesses seemed to be alike, but their meaning was different."

Observer C

The experimenter read to the observer the following summary statement. "For the observer *C* the belief-disbelief con-

sciousness is not a markedly affective experience; in general, belief tends towards pleasantness, disbelief towards unpleasantness. Nor are there pronounced kinæsthetic-organic components, although these sensations may appear and may be integral to the consciousness. The core of belief-disbelief is to be sought in the arrangement and behavior of visual images, which represent both the materials of belief and the pre-existing mental furniture. The visual images are often supplemented by verbal ideas, in which also the belief-disbelief consciousness reaches its terminus. In the case of certainty-uncertainty, consciousness is more strongly affective (certainty being pleasant, uncertainty unpleasant), and the kinæsthetic-organic sensations are more in evidence." The observer accepted this analysis.

The experimenter then asked whether *C* had ever noted anything like an imageless content. The answer was negative, though *C* said that there were many attitudes that she had not been able to analyse, under the conditions and with the time at her disposal; some of these, she remarked, had been analysed by other observers in the work which she herself was conducting. [This work is a general study of the Conscious Attitude.]

Since belief and disbelief had been sufficiently distinguished in the introspective reports, by the characteristics of the visual imagery, no question was asked concerning their discrimination

Observer G

The experimenter read to the observer the following summary statement. "For the observer *G*, belief-disbelief is characterised by moderately strong affective processes, which are normally pleasant for belief and unpleasant for disbelief. The core of the consciousness is constituted of verbal ideas, some of which represent habitual verbal reactions, and others of which are more complicated; these latter are given in a peculiar abbreviated form. There is evidence of kinæsthetic sensation, though this is oftentimes not at all prominent in consciousness; there are occasional visual and auditory images; but, essentially, the whole consciousness is verbal."

The observer accepted this analysis, with a reservation which did away with any necessity for a question regarding the occurrence of imageless thought. He said that the analysis was complete, so far as it went; he found no other contents, imaginal or imageless. But he declared his conviction that belief-disbelief was a course, a mode of conscious processes, specifically determined by the *Aufgabe*; and he added that analysis would not be complete if reference to this pattern or mode of occurrence was omitted.

We are here in presence of a very difficult psychological prob-

lem. We met it also in the early stages of *C*'s introspections, though as time went on *C* laid greater emphasis on the spatial arrangement and mutual relations of her visual images, and less upon the belief-meaning which the separate images seemed earlier to embody. Something of the same sort appears in Ach's sensations of intended movement; sensations of intended finger-movement, it will be remembered, might occur in the eyes. We have already mentioned Störing's state of assurance. Titchener gives an instance of recognition, performed under instruction without a trace of any specific contents (*Thought-processes*, 1909, 179 f., 289 f.). Ach speaks in the same way of volition (*Willens-tätigkeit*, 195, 231; cf. Messer, in *Arch. f. d. ges. Psych.*, viii., 1906, 199 ff.). We need not multiply instances. It seems clear that a certain meaning—intention, recognition, assurance, volition, belief—may be bound up with, incorporated in, a sequence of mental processes which proceed under determination, though there is nothing specific in these processes to serve as the psychological vehicle of that meaning. They go on in a certain way, under the instructions given; and their going on in that way constitutes them will, recognition, belief, etc.

This position, we may add, was several times discussed by the observer *G* and the experimenter, and was accepted by the observer without reservation. It seems especially important to note that *G* finds no trace of imageless contents, since he is precisely of what has been described as the imageless type.

Observer *F*

The experimenter read to the observer the following summary statement. "For the observer *F*, belief-disbelief is not a markedly affective consciousness, though under certain circumstances the affective processes may be moderately strong; the affective reaction is very variable, but normally belief tends to pleasantness, disbelief to unpleasantness. The core of the consciousness consists in the verbal ideas Yes and No, or their equivalents, and in the kinæsthetic sensations aroused by the gesture of nodding and shaking the head. A special organic complex is a frequent component of the belief-consciousness. Oftentimes there is a general kinæsthetic reaction, in which sensations from breathing are perhaps most prominent." This analysis was unhesitatingly accepted by the observer.

The experimenter asked whether *F* had found any trace of an imageless content, and the answer was negative.

CONCLUSION

We may sum up the results of the present enquiry as follows:

(1) The belief-disbelief consciousness, in any form more pronounced than a quasi-mechanical acceptance or rejection, is not of common occurrence in everyday life, and at first is not of common occurrence under experimental conditions. If the instruction is given to observe and report upon this consciousness, however, it appears fairly regularly in response to fitting stimulation.

(2) The belief-disbelief consciousness is not necessarily or regularly an emotional consciousness. Whether or not it is markedly affective in character depends upon the individual constitution of the observer.

(3) The belief-disbelief consciousness may be a straightforward experience, given, *e. g.*, in terms of a general kinæsthetic attitude, or of internal speech and localised kinæsthesia, or of the mutual relations of visual images; or it may be bound up with, incorporated in, a particular consciousness, verbal or visual. In the former case, the contents come to the observer as being, specifically, belief; in the latter case they come to him as the vehicle of belief, which itself finds conscious representation only in the course or mode of occurrence of the contents.

(4) Belief and disbelief are consciousnesses of the same kind.

(5) The certainty-uncertainty consciousness closely resembles that of belief-disbelief, but is in general more strongly affective. Certainty is pleasant; uncertainty, doubt, is unpleasant.

(6) We have supplemented the *Ausfragemethode* in two ways: first, by asking the observers, on the conclusion of the principal experiments, themselves to suggest materials and method for the continued investigation of the belief-disbelief consciousness; and secondly by confronting the observers, at the conclusion of the whole work, with a summary digest of their introspective reports. The first of these supplements served only to confirm our previous conclusions, but it is fair to add that the limits of time forbade us to use it to its fullest extent. The second gave valuable testimony to the correctness of the interpretation put upon the introspections by the experimenter.

APPENDIX

It is clear that the above conclusions hold, primarily, only under the conditions of our experiments. It is therefore important that the reader should know what sort of stimulus-material we employed. We had at first intended to print the sentence or mathematical expression at the head of every piece of quoted introspection. But we found that this procedure would extend our study to an unprintable length. As we did not wish to curtail the introspective evidence—already given only in sample—we were obliged to omit the stimuli; and we now must content ourselves with giving, in this Appendix, a few instances of the sentences, etc., employed.

Single Exposures. Since the whole world, as it exists for an individual consciousness, whether from a practical, theoretical, or æsthetical point of view, has come so to exist through prior mental process, it may be said that there is no objective fact which is not capable of being utilised by the psychologist.

We may say that the elementary or root-function in feeling is susceptibility to pleasure and pain.

Blessed are the meek, for they shall inherit the earth. Blessed are they which do hunger and thirst after righteousness, for they shall be filled. Blessed are the merciful, for they shall obtain mercy. Blessed are the pure in heart, for they shall see God.

The earthquake in Italy caused the death of 200,000 persons.

Wundt published the first edition of his *Physiologische Psychologie* in 1876.

In general, negroes have less power of grasping and exerting pressure than white men; intelligent persons have more power than persons of low intelligence.

The influence of women in all social matters is very great; our present civilisation may be traced to the contributions of women.

That the creature knows its proper food, and may be determined in its knowledge of it by previous impressions, is regarded as the first and primal indication of the presence of mental life.

Education is, in the last resort, a sort of hypnotism.

A hawk in South America could tell the time; it went every day to the monastery at the precise hour when food was distributed to the poor.

There have been many reformers, but none more radical than Rousseau; for he advocated the overthrow of civilisation and the return to a state of nature.

In truth, it was not language that generated the intellect; it is the intellect that formerly invented language; and even now the newborn child brings with him into the world far more intellect than talent for language.

- Moral instruction should touch the hearts of the pupils.

Though we may by a device of abstraction distinguish between a mental process and its results, a producing operation and its product, we must remember that these are only two aspects of the same phenomenon.

In the heat of battle a man may be wounded and, for a time, be wholly insensible of his pain, because the intense engagement of the system—both brain and muscles—precludes the diffusion of impulses in the usual channels.

Urteile sind Bewusstseinsvorgänge auf welche die Prädikate richtig oder falsch eine sinngemässe Anwendung finden.

Coeducation is one of the best means for elevating the character of our boys and girls.

A can do a piece of work in 5 days, *B* can do it in 4 days. How long will it take *A* and *B* to do the work together? Answer: 2 2/3 days.

$$897 + 789 + 987 = 2673$$

$$\frac{42a^3 - 30a^2x}{35ax^2 - 25x^3} = \frac{6a^2}{5x^2}$$

Paired exposures. (a) Man is the creator of God. (b) Living men can communicate with the spirits of the dead.

(a) Even the purest well-water contains eleven million bacteria to the cubic inch. (b) The people on Mercury are trying to attract our attention by intense light-signals.

(a) Within fifty years we shall see the establishment of a democratic form of government in Russia. (b) Newton saw the law of gravitation in the fall of an apple.

(a) Parallel lines, however far produced, never meet. (b) Judgment is only a train of fluctuating ideas ending in a clear and steady idea.

(a) Our capacity of memory is determined by the number of cells

in our cerebrum. (b) Too great freedom of marriage means too great ease of divorce.

(a) Clear ideas are not necessarily persistent ideas. (b) There is no inborn difference between the different races; the existing differences are due to social heredity.

(a) Take alcohol out of human life, and the number of criminals will be greatly decreased. (b) Children love their parents for the pleasure they provide for them.

(a) There are but few cases in which desire is not accompanied by pain. (b) Examine the desires in your mind at this present moment, and you will find little else than pleasure.

(a) If there were no attraction between the earth and the sun, the mechanical pressure of light, thrown upon the whole surface of the earth, would drive it away with enormous rapidity. (b) Light is the vibration of the omnipresent ether.

(a) Corsets that embrace the waist with a grip that tightens respiration into pain, and skirts that weight the hip with heavier than maternal burdens, cause grievous maladies. (b) Clever children delight in acquiring knowledge, and feel pride in outstripping their competitors; but they must be restrained in their activity, more especially as it too often happens that mental precocity is associated with feeble bodily powers.

A PRELIMINARY INTROSPECTIVE STUDY OF THE ASSOCIATION-REACTION CONSCIOUSNESS.

By L. R. GEISSLER

The following report appears in advance of a more exhaustive introspective analysis of the "complex," begun by Miss E. T. Burr and the present writer in the Psychological Laboratory of Cornell University during the Summer Session of 1910. This study is to be continued during the present year. The experiments were conducted by Miss Burr; and the author, whose introspections are here reported, was one of the four principal observers. The material consisted of pairs of pictures or stories, presented to the observer at the beginning of the hour so that he could choose the one member of the pair without the experimenter's knowing which had been selected. The reaction-time was taken by a stopwatch, registering fifths of seconds. After every association the observer gave a detailed description of his state of mind and its contents, from the "ready-signal" until he had given his association-word. He was also asked to adhere as closely as possible to the temporal sequence of his experiences, and to notice in particular the background processes.

The introspections of *G* seem to be significant for three special points. (1) They indicate the general frame of mind during an association-reaction experiment. (2) They show that some of the mental processes occurring in *G*'s reactions are entirely indifferent to the presence or absence of a "complex." And (3) they furnish a clue to the nature and symptoms of certain kinds of "complexes."

The ideational type of *G* is predominantly verbal-motor. He is so largely dependent upon his general bodily attitude that he is not in the right mood toward the experiment, that is, in the attitude of passive, impartial indifference, until he has assumed the exact position in the chair which he happened to take at the beginning of the hour. His mental attitude at the moment of the "ready-signal" was as a rule one of calm, expectant attention, and indifference to extraneous stimuli; in the background was a vague sense of being "on guard." No further details of this state can as yet be given. Most of the insignificant stimulus-words made no material change in this state. The focus of attention shifted quickly

from the perception of the stimulus to the pronunciation of the reaction-word. The latter frequently appeared with reflex-like regularity; 45 out of *G*'s 91 irrelevant reactions took less than 1.5 sec., with an average of 1.2 sec. and a m. v. of .1 sec. In all these cases the general pattern of consciousness was that of a sharp focus and a very vague background. A few times only a sort of "blank" was experienced, as it is described, for example, in the following record: "verbally repeated stimulus-word ('East-Side') to myself, then short, empty pause, empty staring, fixation of objects opposite me, but nothing clear in mind for short moment, muscular sensations from movement of eyes in easterly direction, and reaction-word 'New York' came immediately" (time, 2.4 sec.). The reaction-times for the "blank" experiences average about 2 sec. Sometimes expectation is so intense as to anticipate a stimulus-word before it is completely heard; *e. g.*, "crisis" was expected to be "Christ," and "charity" to be "chariot." In such cases the expected word is usually given as the observer's reaction.

While with the insignificant words a great uniformity of the attentive state of mind thus prevailed, in the case of significant stimuli, the conditions were extremely labile. Here the observer was often unable exactly to describe his attitude; he "felt lost," or was "in a state of suspense," or he "had no memory or recognition of the stimulus-word until a reaction word occurred to him." Several times *G* had prepared himself for certain "dangerous" words by thinking beforehand of a "safe" reaction. But at the proper time this preparation failed to work, because some other mental process, perhaps an image or a strong feeling, successfully interfered with or delayed the pre-arranged reaction. Quite frequently the stimulus-word was repeated, more and more automatically, so that it gradually dropped out of the focus into the background, while nothing else took its place. The consequence was a "blank," which may have lasted sometimes as long as a second. The recovery was very sudden; a new idea appeared and led to an almost explosive reaction. It is difficult to describe this state more fully without mentioning its contents, which will be done in the third section. As the experiments were never continued for more than 50 minutes, *G* never experienced any signs of fatigue.

As regards the mental processes which are indifferent to the presence of a "complex," it has already been pointed out that *G* was, as a rule, entirely oblivious to his surroundings. Only in 8 of the insignificant and in 3 of the significant cases did environmental stimuli, such as visual objects or sounds, influence him by suggesting reaction-words. As *G*

is a very poor visualizer, the frequency of his visual images in these experiments seems remarkable. They were, of course never more than vague, except in two significant reactions, where he had seen some striking picture that had made a deep impression upon his mind. Most of his visual images were of a symbolic character; *e. g.*, a whole orchestra was represented by "a black spot with white specks in it," indicating the men in dress-suits and the white front of their shirts. Verbal ideas were by far the most numerous, ranging in degree from actual speech-movements to what *G* calls his "verbalizing tendency," in which sometimes a mere upward pressure of the tongue against the roof of the mouth indicates the beginning of a word or letter. In many cases the stimulus-word is simply supplemented to form a new (reaction-)word; *e. g.*, "beauty" is completed by "ful" to stand for "beautiful." No less frequent are alliterations and synonyms, while rhymes and even nonsense-rhymes occur at times. These verbal processes are distributed over insignificant and significant cases with relatively equal frequency. Closely related to *G*'s verbal ideas are the kinæsthetic elements of his consciousness. He frequently experienced strains indicating "a rigidity of the mouth and other speech-organs." Sometimes the mere "initial movement to close lips" leads to pronouncing a reaction-word beginning with *b* or *p*, such as "picture" or "boy." *G* often speaks of "a tendency to move," "to turn head," "to say something," or the like. In one case he had successively "several tendencies to move lips, but no anticipation of what was to come out," and in another case he made "several tentative lip and eye-movements." The latter are especially important for his whole mental make-up, as they frequently carry or represent to him the logical meaning of an idea. The symbolic character of the eye-strains and movements is, *e. g.*, brought out in the following introspections: "I had eye-movements toward place on page where stimulus-word is found," or: "on my lips definitely the word 'sesame' (stimulus was "open"), was inhibited, then word "door" with reference to China, looking sideways to left, like studying map of whole world where China is on left side." (This map had been used in my high school course.) In a good many cases *G* could not indicate the psychological processes underlying his thoughts, meanings, and references. Several reasons for this fact may be inferred from his introspections. Sometimes the thoughts were too fleeting or too vague; *e. g.*, "the thought came like a flash: that is good, safe," meaning of course that the reaction-word would not betray him. Or, sometimes, too many ideas occurred at once; "lots of things crossed my mind; . . . ideas are

there in some sort of vague awareness only." Again, the thoughts may form a chain of syllogisms, the final conclusion alone being present in a more concrete form. Usually the thoughts were interfered with by a strong emotional coloring, as in the following case: Stimulus: Spanish, Reaction: American, time: 1.9 sec. "I vaguely thought of Spanish-American war, mostly verbal, with thought of Cuba, a reference which seemed emotional, I had taken sides with the Spaniards." Quite frequently the reaction-word is accompanied in the background of consciousness by a thought of its justification, which is probably the result of the *Aufgabe* "not to betray myself." This *Aufgabe* acts, of course, like a determining tendency; it was not often conscious. Even when it was experienced, it was not always present in the same form or to the same degree. For example, the stimulus-word "reddish," referring to the hair of a girl in a story, led to the reaction of "rabbit," after 3.5 sec., through the following process: "association is due to thought that rabbit is spelt with 2 b's just as reddish has 2 d's, mostly verbal. After long, unpleasant inhibition of speech-organs, rigidity of lips and mouth, frowning and squinting, 'rabbit' came with thought: 'what is the difference? No need of sensible word.' This reasoning occurred while and after saying the word 'rabbit.'" It is worth notice that the "tendency to say something quickly" was stronger in the significant cases, while "a determination to say something sensible" prevailed with irrelevant stimuli. But this is not a frequent or a very consistent phenomenon. The perseverative tendency exerted its influence rather more frequently. Words that had occurred previously in introspections, or as stimuli, or as associations, were employed once or twice again during the same hour, usually with the recognition that they had been used before. The main reason why all these various different mental processes are grouped together is that they seem to have no special significance for the hidden complex, as they occur at least just as frequently, and often more so, with insignificant as with significant association reactions.

On the other hand, the mental processes which are apt to be connected with a "complex" appear only very rarely with indifferent words. It is much more difficult to make a sharp analysis of these symptoms and to enumerate separate processes, because as a rule the complex is felt merely as a general mental and physiological inhibition. The former is experienced as a "blank" or interruption of the regular course of mental life, as an obstacle or block in the stream of consciousness. G pictures it to himself in cross-section like the dry passage which the Israelites used to cross the Red Sea.

He finds three stages in the mental aspect of inhibition. Immediately upon hearing the significant word, consciousness is crowded with ideas in the focus and swamped by a strong affection. This stage passes more or less gradually into an empty staring, waiting, seeking, with practically no focus and all background. Finally a single idea comes, shooting to a head, that is, to focal clearness and discharging itself into movements of speech. It thus overcomes the inhibition or brings about its *Auflösung*, and initiates a state of bodily relief accompanied by a sudden change from the previous strong unpleasantness to a less strong pleasantness. These three stages seem to occupy various lengths of time and reach different degrees of intensity. The physiological side of inhibition includes such phenomena as holding of the breath, upward pressure of the diaphragm, drawing inward of the abdominal region, and rigidity of the laryngeal organs, tongue-tightness, and open mouth. The strains and pressures coming from these different parts of the body make up a large part of the background that is left during the second stage, the "empty staring and waiting period," during which one feels utterly helpless.

The feelings or emotions which in *G* characterize the presence of a "complex" are of two kinds, excitement and anxiety on the one hand, and relief on the other. The excitement seems to be mainly a quick, flash-like, but strongly unpleasant complex of faint visceral sensations, while the anxiety takes a certain time to develop, gradually becoming more and more unpleasant and at the same time confining itself more and more to the sensations due to muscular rigidity. There does not seem to be any regularity in *G*'s records about the occurrence of the one or the other emotion. Both emotions are subject to decay after several recurrences in the same hour. The visceral complex loses in intensity and excitement gradually wears off; and similarly, anxiety slowly disappears with the shortening of the empty waiting period. The change from excitement or anxiety to relief sets in with the appearance of a new idea. Relief is characterized, according to *G*'s observations, by the resumption of normal breathing, beginning usually with a long exhalation which leads sometimes to a whispered "oh!" and by the return of the muscles from the strained contraction to a normal equilibrium. This loosens the vocal organs and makes the pronunciation of the reaction-word possible. The speaking and hearing of the voice is moderately pleasant. In a few rare instances a "slight feeling of relief at easy reaction" is experienced, even in cases of insignificant stimuli, while there is no preceding excitement or anxiety.

These emotions are not the only symptoms of a complex. Quite frequently there are also references to the picture seen or the story read. Some of them consist of images, movements, or kinæsthetic sensations connected with the study of the picture or story, and may occur with varying degrees of completeness and vagueness. The other kind of references to the complex is more puzzling. They read like the following: "memory of end of story," or: "conscious of story," again: "aware that my association was taken from story," "the whole story was present, can't tell how," or this: "I knew stimulus-word had nothing to do with picture." The last quotation shows that some of these references appeared even when the stimulus was not intended to be significant. Probably these hints of the complex are signs of the general attitude of "being on guard," and should be grouped with the frequent attempts at justification of the given reaction-word. It may be mentioned that *G* did not always successfully maintain this attitude, but such cases are too rare to deserve fuller treatment.

What, then, is the "complex?" According to these results it is a strongly unpleasant group of ideas (connected with the concealed object), reinforced by certain organic sensations, and characterized by a quick change from focal crowdedness through a momentary blankness to the dominance of a single focal idea. It may, perhaps, be surprising to some readers that nothing has been said about any of the specifically Freudian complexes, such as the wish-complex, the sexual-complex, or the father-complex. Probably the conditions of these experiments were not favorable for their occurrence; the author cannot deny their existence in his dream life. It is hoped that future work will throw more light upon these problems.

A BIBLIOGRAPHY OF THE SCIENTIFIC WRITINGS OF WILHELM WUNDT

By E. B. TITCHENER and L. R. GEISLER

(Second supplementary list)

The following is the second supplement to our Wundtian bibliography (this *Journal*, xix., 1908, 541; xx., 1909, 570). Information regarding the early titles was kindly transmitted to us, on Professor Wundt's behalf, by Professor Wirth of Leipzig; we have also to thank Dr. Okabe, of Cornell University, for verifying references.

Three popular articles—*Ueber den Blick*; *Ueber die Mimik des Menschen*; *Ueber die Zeit*—were published by Professor Wundt, about the years 1857-1860, in Karl Gutzkow's *Unterhaltungen am häuslichen Herd* (Leipzig, Brockhaus). We have not found this periodical in America, and we shall be grateful for any assistance that readers of the *Journal* may be able to render us in the matter.

1853

(1) *Ueber den Kochsalzgehalt des Harns*. Erdmann und Werther's *Journal für praktische Chemie*, lix. (Jahrgang 1853, zweiter Band), Heft 5 und 6, 354-359.

1855

(1) *Versuche über den Einfluss der Durchschneidung der Lungenmagennerven auf die Respirationsorgane*. Müller's *Archiv für Anatomie, Physiologie und wissenschaftliche Medicin*. Jahrgang 1855, 269-313.

1856

(1) [Add:] Druck von G. Mohr. pp. 28. Mit einer Steindrucktafel.

1857

(3) *Ueber den Einfluss hydrotherapeutischer Einwicklungen auf den Stoffwechsel*. Vogel, Nasse und Beneke's *Archiv des Vereins für gemeinschaftliche Arbeiten zur Förderung der wissenschaftlichen Heilkunde*, iii., 35-43.

1861

(6) *Ueber die persönliche Differenz zwischen Gesichts- und Gehörsbeobachtungen mit Rücksicht auf die astronomischen Durchgangsbeobachtungen*. *Tageblatt der Naturforscherversammlung in Speyer*, 17 Sept., 1861. Astronomische Section.

1866

(3) *Ueber Haeckels generelle Morphologie der Organismen*. Goesch's Kritische Blätter für wissenschaftliche und praktische Medizin, 1866, nos. 2 and 3.

1887

(7) *Zum "ethischen Evolutionismus."* Eine Entgegnung [against H. Sommer]. Preussische Jahrbücher, ed. H. von Treitschke and H. Delbrück, lix., Jan.-June 1887, 5tes Heft, 478-485.

1908

(6) *Märchen, Sage und Legende als Entwicklungsformen des Mythos*. Dieterich's Archiv für Religionswissenschaft, xi., 200-222.

1909

(7) *Einleitung in die Philosophie*. Fünfte Auflage. Mit einem Anhang, etc. Large 8vo. Leipzig, W. Engelmann. pp. xviii., 471.

1910

(1) *Grundzüge der physiologischen Psychologie*. Sechste, umgearbeitete Auflage. Vol. ii., with 167 illustrations and indices. Large 8vo. Leipzig, W. Engelmann. pp. viii., 782.

(2) *Das Institut für experimentelle Psychologie zu Leipzig*. With one illustration. Psychologische Studien, v., Heft 5 und 6, 22 Februar 1910, 279-293. Reprint of 1909 (6).

(3) *Logik und Psychologie*. Zeitschrift für pädagogische Psychologie, Pathologie und Hygiene, xi., Heft 1, 1910, 1-18.

(4) *Die Prinzipien der mechanischen Naturlehre. Ein Kapitel aus einer Philosophie der Naturwissenschaften*. Zweite, umgearbeitete Auflage der Schrift: *Die physikalischen Axiome und ihre Beziehung zum Kausalprinzip*. Large 8vo. Stüttgart, F. Enke. pp. xii., 217.

William James

The death of William James has taken from American psychology its most distinguished representative, and from American psychologists the colleague held in highest and most affectionate regard.

Professor James' productive period as a writer of books was late in beginning. His first extensive work, the "Principles of Psychology" (1890), was not published till its author was forty-eight years old. After it came, in the remaining twenty years of his life, his "Text-book of Psychology" in 1892, "The Will to Believe" in 1897, "Talks to Teachers on Psychology" in 1898, "Human Immortality" in 1899, "The Varieties of Religious Experience" in 1902, "Pragmatism" in 1907, "A Pluralistic Universe" in 1908, and "The Meaning of Truth" in 1909. His honors, following on his works, came likewise late, but included degrees from Padua, Edinburgh, Oxford, Durham and Geneva abroad, and from Princeton and Harvard in this country. He was Lowell Institute lecturer in Boston, Gifford lecturer at the University of Edinburgh, and Hibbert lecturer at Oxford. He was a corresponding member of learned societies in France, Germany, Denmark, Italy and Great Britain, and was a member of our own National Academy of Sciences.

Abundant honors and abundantly deserved! And yet, to many American psychologists, the loss to their science and to philosophy will be overshadowed by their feeling of personal loss. Beyond the immediate circle of his colleagues and pupils at Harvard, many were happy in his friendship, and to many of the younger workers, in particular, he had given never-to-be-forgotten words of encouragement. No American psychologist has ever been held in like esteem, and none has carried through criticism and controversy such universal and affectionate regard. This was the natural tribute paid to powers of the highest order, exercised unselfishly; to a chivalrous passion for fair play, which often placed Professor James on the less popular side of disputed questions; and to a courtesy that assumed, in every opponent, standards and ideals equal to his own.

James, the psychologist, will long be held in high repute. Yet a growing science leaves even its ablest representatives behind; and despite the originality of his thought, the erudition that he so lightly carried, his consummate skill in inner observation, and his literary charm, the work that he has bequeathed to us will presently be superseded. But be that as it may,—the modest sincerity of the man, his human friendliness, and his crusading spirit were of the best that the world affords, and can never be outgrown. The EDITORS.

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